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RACE, REGION, AND VOTE CHOICE IN THE 2008 ELECTION: IMPLICATIONS FOR THE FUTURE OF THE VOTING RIGHTS ACT

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TABLE OF CONTENTS

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The election of an African American as President of the United States has raised questions regarding the continued relevance and even constitutionality of various provisions of the Voting Rights Act (VRA). Barack Obama's apparent success among white voters in 2008 has caused some commentators to question the background conditions of racially polarized voting that are key to litigation under section 2 of the VRA. His success in certain states, such as Virginia, has also raised doubts about the formula for coverage of jurisdictions under section 5 of the VRA. This Article examines the data from the 2008 primary and general elections to assess the geographic patterns of racial differences in voting behavior. The data suggest that significant differences remain between white and minority voters and among jurisdictions that are covered and not covered by section 5 of the VRA. These differences remain even when controlling for partisanship, ideology, and a host of other politically relevant variables. This Article discusses the implications of President Obama's election for legal conceptions of racially polarized voting and for decisions concerning which jurisdictions section 5 ought to cover.

INTRODUCTION: THE VOTING RIGHTS ACT AND THE ELECTION OF AN AFRICAN AMERICAN PRESIDENT

When Congress passed the Voting Rights Act of 1965[1] (VRA), the election of an African American President was inconceivable. Even when Congress reauthorized expiring provisions of the VRA in 2006,[2] such an election appeared a distant possibility. Now, as the Supreme Court has cast constitutional doubt on the reauthorized VRA,[3] what once seemed impossible or unlikely has become concrete and

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[1] Pub. L. No. 89-110, 79 Stat. 437 (codified as amended at 42 U.S.C. § 1973 (2006)).
[2] Fannie Lou Hamer, Rosa Parks, and Coretta Scott King Voting Rights Act Reauthorization and Amendments Act of 2006, Pub. L. No. 109-246, 120 Stat. 577.
[3] *See* Nw. Austin Mun. Util. Dist. No. One v. Holder, 129 S. Ct. 2504 (2009) [hereinafter *NAMUDNO*].

real: a member of the racial minority for whom the VRA was written occupies the Oval Office.

It is unsurprising, then, that the election of Barack Obama has led some commentators to question both the relevance[4] and the constitutionality[5] of the VRA. If an African American candidate can win a majority of the national vote and even do better than previous Democratic nominees among white voters in states as varied as Colorado, Indiana, and Virginia, do the fundamental assumptions underlying the VRA need to be rethought? In particular, does the 2008 election signal a fundamental shift in race-based patterns of voting behavior, such that the geographic reach of section 5 of the VRA[6] or the primacy of racially polarized voting in analysis under section 2 of the VRA[7] requires updating?

In this Article, we assess the patterns of race and political preference in the 2008 election and consider their relevance for the meaning and constitutionality of the VRA.[8] The exit polls and election returns suggest that the 2008 election did not represent a fundamental shift in national patterns of race and vote choice. However, these national patterns mask great variation at the state and county level. In particular, Obama's relative success among white voters, as compared to John Kerry four years earlier, varied greatly by region. In the Deep South, Obama actually did worse than Kerry among white voters. Nationally, Obama did much better among African Americans and Latinos, with both groups turning out to vote at higher rates and giving him a higher proportion of their votes.

We view these findings as principally a response to the charges that the 2008 election represented a fundamental transformation in voting patterns relevant to the VRA. However, we recognize that this evidence bears on an ongoing debate concerning the relevance of racially

[4] *E.g.*, Abigail Thernstrom & Stephan Thernstrom, Op-Ed., *Racial Gerrymandering Is Unnecessary*, WALL ST. J., Nov. 11, 2008, at A15.

[5] *E.g.*, Brief for Appellant at 2–3, *NAMUDNO*, 129 S. Ct. 2504 (No. 08-322), 2009 WL 453246.

[6] 42 U.S.C. § 1973c (2006).

[7] *Id.* § 1973b.

[8] This Article expands upon an amicus brief we filed on behalf of neither party in *NAMUDNO*. *See* Brief for Nathaniel Persily et al. as Amici Curiae on Behalf of Neither Party, *NAMUDNO*, 129 S. Ct. 2504 (No. 08-322), *available at* http://www.law.columbia.edu/null/download?&exclusive=filemgr.download&file_id=151457. The brief was mentioned by Justice Kennedy at the oral argument, *see* Transcript of Oral Argument at 55–56, *NAMUDNO*, 129 S. Ct. 2504 (No. 08-322), *available at* http://www.supremecourtus.gov/oral_arguments/argument_transcripts/08-322.pdf, and cited by numerous commentators at the time of the hearing. *See, e.g.*, Robert Barnes, *High Court to Weigh Relevance of Voting Law in Obama Era*, WASH. POST, Apr. 1, 2009, at A1; Adam Liptak, *Review of Voting Rights Act Presents a Test of History v. Progress*, N.Y. TIMES, Apr. 28, 2009, at A16; Jeffrey Toobin, *Voter, Beware*, NEW YORKER, Mar. 2, 2009, at 19; Posting of Linda Greenhouse to The Supreme Court Breakfast Table, http://www.slate.com/id/2220927/entry/2221036 (June 22, 2009, 13:39 EST).

polarized voting patterns, particularly to the constitutionality of section 5 of the VRA,[9] as well as perhaps to the continued operation of section 2. In Part I we discuss the importance of racially polarized voting patterns for the meaning of section 2 and the constitutionality of section 5 of the VRA. Part II presents background data from 1984 to 2004 against which we can judge any transformation that took place in the 2008 election. The data show persistent differences between minorities and whites in their candidate preferences and between the preferences of whites in the covered and noncovered states.[10] Part III presents the data from the 2008 general and primary elections and analyzes Obama's relative success in the states covered and not covered by section 5 of the VRA. We pay particular attention to differences in the behavior of white voters between 2004 and 2008. We analyze exit poll results, aggregated election returns, and other survey data to conclude that the differences in candidate preferences in 2008 between whites in the covered and noncovered states cannot be completely explained by partisan, ideological, or demographic differences. Such factors, especially party identification, can account for the differences in the voting behavior of whites across covered and noncovered jurisdictions observed in 2004, but these factors do not account fully for the differences observed in 2008. The Conclusion discusses the implications of our findings for cases going forward.

We should admit up front to an ambivalence as to the role of the 2008 election in current debates over the VRA. We believe that the VRA, and especially the coverage formula for section 5, needs to be updated or revised specifically to provide greater protection for minority voting rights.[11] However, we also believe the VRA continues to represent a constitutional exercise of congressional power under the Fourteenth and Fifteenth Amendments.[12] Congress provided a sufficient record of threats to minority voting rights in the covered jurisdictions to justify the continued operation of the law even in its current form.[13] The results of a single presidential election, whatever they may show, do not shake our belief either in the necessity of reform or

[9] *See NAMUDNO*, 129 S. Ct. at 2526 (Thomas, J., concurring in the judgment in part and dissenting in part).

[10] *See* CIVIL RIGHTS DIV., U.S. DEP'T OF JUSTICE, SECTION 5 COVERED JURISDICTIONS, *available at* http://www.usdoj.gov/crt/voting/sec_5/covered.php (listing as covered states: Alabama, Alaska, Arizona, Georgia, Louisiana, Mississippi, South Carolina, Texas, and Virginia).

[11] This topic is not the subject of this article, but one of us has written extensively on it. *See* Nathaniel Persily, *The Promise and Pitfalls of the New Voting Rights Act*, 117 YALE L.J. 174, 222–23 (2007).

[12] *See* Nathaniel Persily, *The Constitutional Relevance of Alleged Legislative Dysfunction*, 117 YALE L.J. POCKET PART 256 (2008), http://www.yalelawjournal.org/images/pdfs/678.pdf.

[13] *See* Persily, *supra* note 11, at 192–216.

in the constitutionality of the currently flawed law. Nevertheless, because the unprecedented 2008 election occurred in the midst of a constitutional challenge to section 5 of the VRA, the election caused handwringing over the importance of election results for the constitutionality and desirability of the VRA.

Proponents and opponents of the VRA alike will seek to find support in the data presented here. For those supportive of the VRA, we demonstrate the persistence of race-based differences in presidential voting patterns, especially in the states covered by section 5 of the VRA. There can be no question that the gap in vote preferences between white and minority voters is larger in the covered states than in the noncovered states, as a group. These differences, at least with respect to whites' support in 2008, cannot be explained away by the fact that the whites in the covered states are more Republican, more religious, or more conservative.

All that considered, there is great diversity among the states, regardless of coverage status, in the racial gap in voter preferences and the propensity of whites to vote for Obama. Although whites in the covered states did not, as a group, vote in larger shares for Obama, that is because in some states (such as Virginia) he did better among whites compared to 2004, while in other states (such as Mississippi, Alabama, and Louisiana) he did worse. Although he generally improved over Kerry's vote share in the noncovered states, in Arkansas Obama did worse among whites. And, in the end, the fact that Obama received a share of the white vote in the covered states that was comparable to that received by John Kerry, a white candidate, only four years prior signals how far we have come since the passage of the VRA.

I. THE POTENTIAL LEGAL IMPLICATIONS OF THE 2008 ELECTION

Whether one takes the most anemic view of voting rights, as limited to the casting and counting of ballots,[14] or even the more capacious view, as concerning anything affecting the "power" of one's vote,[15] candidate success does not bear ineluctably on questions con-

[14] *See, e.g.*, Holder v. Hall, 512 U.S. 874, 893, 914–15 (1994) (Thomas, J., concurring in the judgment) (arguing that the VRA ought to be limited to barriers to participation and not to extend to dilution); City of Mobile v. Bolden, 446 U.S. 55, 65 (1980) (plurality opinion) (concluding that the Fifteenth Amendment did not concern dilution); *id.* at 84 n.3 (Stevens, J., concurring in the judgment) (characterizing plurality opinion as concluding that the Fifteenth Amendment "applies only to practices that directly affect access to the ballot and hence is totally inapplicable to the case at bar").

[15] *See, e.g.*, 42 U.S.C. § 1973*l*(c)(1) (2006) ("The terms 'vote' or 'voting' shall include all action necessary to make a vote effective in any primary, special, or general election, including, but not

cerning the abridgement of voters' rights. Whether specific candidates win or lose does not necessarily speak to the question of whether voters' rights were respected in the electoral process.

As voting law has moved from a preoccupation with access and participation to inquiries concerning dilution, however, the relative success of minority-preferred candidates has become a central focus of courts and litigants attempting to assess voting rights progress or lack thereof.[16] Successful claims of illegal vote dilution under section 2 of the VRA depend on a demonstration that racially polarized voting patterns hinder the election of minority-preferred candidates.[17] Moreover, in the findings of the newly reauthorized section 5, Congress expressly mentioned racially polarized voting in the covered jurisdictions as one of the justifications for the law.[18] At the end of this Article, we return to the questions of whether and when polarization should be relevant for voting rights law, but for present purposes, we simply note the centrality of racial bloc voting to the statutes of concern and the historical and legal debates.

A. The Role of Racially Polarized Voting in Litigation Under Section 2 of the VRA

> *Some commentators suggest that racially polarized voting is waning — as evidenced by, for example, the election of minority candidates where a majority of voters are white. Still, racial discrimination and racially polarized voting are not ancient history. Much remains to be done to ensure that citizens of all races have equal opportunity to share and participate in our democratic processes and traditions; and [section] 2 must be interpreted to ensure that continued progress.*[19]

limited to, registration, listing pursuant to this subchapter, or other action required by law prerequisite to voting, casting a ballot, and having such ballot counted properly and included in the appropriate totals of votes cast with respect to candidates for public or party office and propositions for which votes are received in an election."); Allen v. State Bd. of Elections, 393 U.S. 544, 566, 569–70 (1969) (holding section 5 of the VRA is applicable to any law that affects the weight of a citizen's vote, including dilutive systems of representation).

[16] *See generally* Heather K. Gerken, *Understanding the Right to an Undiluted Vote*, 114 HARV. L. REV. 1663 (2001).

[17] *See* 42 U.S.C. § 1973(b) ("The extent to which members of a protected class have been elected to office . . . is one circumstance which may be considered [in assessing whether] the political processes leading to nomination or election . . . are not equally open to participation by members of a [protected] class of citizens.").

[18] Fannie Lou Hamer, Rosa Parks, and Coretta Scott King Voting Rights Act Reauthorization and Amendments Act of 2006, Pub. L. No. 109-246, § 2(b)(3), 120 Stat. 577 ("The continued evidence of racially polarized voting in each of the [covered] jurisdictions . . . demonstrates that racial and language minorities remain politically vulnerable, warranting the continued protection of the Voting Rights Act of 1965.").

[19] Bartlett v. Strickland, 129 S. Ct. 1231, 1249 (2009) (plurality opinion) (citations omitted).

"Racially polarized voting" or "racial bloc voting" is a term of art in voting rights law.[20] The concept has its genesis in racial vote dilution cases brought under the Fourteenth Amendment.[21] It played an important role in the legislative history of the 1982 amendments to the VRA,[22] and then became the touchstone of the Supreme Court's test from *Thornburg v. Gingles*[23] for proving illegal vote dilution. If the 2008 election revealed decreasing rates of racial polarization in the electorate — certainly a plausible hypothesis given Obama's success — the election results might indicate that section 2 cases would be more difficult to win in the future.

Section 2 of the Voting Rights Act prevents jurisdictions from enacting voting laws that deny or abridge the right to vote "on account of race."[24] It specifies that a violation of the law occurs when, based on the "totality of the circumstances,"

> it is shown that the political processes leading to nomination or election . . . are not equally open to participation by members of a [protected] class of citizens . . . in that its members have less opportunity than other members of the electorate to participate in the political process and *to elect representatives of their choice*. The extent to which members of a protected class have been elected to office in the State or political subdivision is one circumstance which may be considered . . . .[25]

Section 2 litigation is almost exclusively concerned with vote dilution by way of at-large systems of representation or redistricting plans.[26] When successful, it usually leads courts to create majority-minority districts that give minority voters a greater chance of electing their preferred candidates.[27]

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[20] *See* Thornburg v. Gingles, 478 U.S. 30, 53 n.21 (1986) ("'[R]acial polarization' exists where there is 'a consistent relationship between [the] race of the voter and the way in which the voter votes,' or to put it differently, where 'black voters and white voters vote differently.' We, too, adopt this definition of 'racial bloc' or 'racially polarized' voting." (alteration in original) (citations omitted)). *See generally* Samuel Issacharoff, *Polarized Voting and the Political Process: The Transformation of Voting Rights Jurisprudence*, 90 MICH. L. REV. 1833 (1992).

[21] *See, e.g.*, White v. Regester, 412 U.S. 755 (1973).

[22] S. REP. NO. 97-417, at 29 (1982), *reprinted in* 1982 U.S.C.C.A.N. 177, 206.

[23] 478 U.S. 30 (1986).

[24] 42 U.S.C. § 1973(a) (2006).

[25] *Id.* § 1973(b) (emphasis added).

[26] *See, e.g.*, Bartlett v. Strickland, 129 S. Ct. 1231, 1238–39 (2009) (plurality opinion); League of United Latin Am. Citizens v. Perry, 548 U.S. 399, 409–10 (2006) [hereinafter *LULAC*]; Johnson v. De Grandy, 512 U.S. 997, 1000 (1994); Growe v. Emison, 507 U.S. 25, 27 (1993); Thornburg v. Gingles, 478 U.S. 30, 34 (1986).

[27] *See* Holder v. Hall, 512 U.S. 874, 897 (1994) (Thomas, J., concurring in the judgment) ("Perhaps the most prominent feature of the philosophy that has emerged in vote dilution decisions since *Allen* [*v. State Bd. of Elections*, 393 U.S. 544 (1969)] has been the Court's preference for single-member districting schemes, both as a benchmark for measuring undiluted minority voting strength and as a remedial mechanism for guaranteeing minorities undiluted voting power.").

The Supreme Court's decision in *Gingles* established a threshold test for demonstrating that an at-large system or districting arrangement dilutes minority votes.[28] If a minority group is large enough to constitute a majority in a single-member district,[29] votes cohesively, and is systematically outvoted by whites, then it will likely have a vote dilution claim under section 2.[30] The structure of an at-large scheme or districting arrangement coupled with the voting behavior of each racial group, under this view, may dilute the votes of the racial minority.

Demonstrating "racially polarized voting" is, therefore, the key to proving a violation of section 2[31] (even though courts require that plaintiffs prove the so-called "Senate Factors"[32] as well). A plaintiff

[28] *Gingles*, 478 U.S. at 46–50.

[29] *See Strickland*, 129 S. Ct. at 1241–46 (plurality opinion) (making clear that the *Gingles* test requires minorities to be large enough to constitute a voting age majority in a single member district).

[30] *See Gingles*, 478 U.S. at 50–51. Of course, the ingredients for a successful vote dilution lawsuit are more complicated than that and include proving the so-called "Senate Factors." *See id.* at 43–46; *see also Johnson*, 512 U.S. at 1000 (holding that "proportionality" is a factor counting in favor of a districting plan's legality). *See generally* Ellen Katz et al., *Documenting Discrimination in Voting: Judicial Findings Under Section 2 of the Voting Rights Act Since 1982: Final Report of the Voting Rights Initiative, University of Michigan Law School*, 39 U. MICH. J.L. REFORM 643, 675–732 (2006) (describing Senate Factors as litigated in the lower courts).

[31] *See Gingles*, 478 U.S. at 55–58.

[32] The "Senate Factors" refer to the list of factors necessary to prove a vote dilution claim as delineated in the Senate Report accompanying the 1982 Amendments to the Voting Rights Act. They include:

> 1. the extent of any history of official discrimination in the state or political subdivision that touched the right of the members of the minority group to register, to vote, or otherwise to participate in the democratic process;
>
> 2. the extent to which voting in the elections of the state or political subdivision is racially polarized;
>
> 3. the extent to which the state or political subdivision has used unusually large election districts, majority vote requirements, anti-single shot provisions, or other voting practices or procedures that may enhance the opportunity for discrimination against the minority group;
>
> 4. if there is a candidate slating process, whether the members of the minority group have been denied access to that process;
>
> 5. the extent to which members of the minority group in the state or political subdivision bear the effects of discrimination in such areas as education, employment and health, which hinder their ability to participate effectively in the political process;
>
> 6. whether political campaigns have been characterized by overt or subtle racial appeals;
>
> 7. the extent to which members of the minority group have been elected to public office in the jurisdiction.
>
> Additional factors that in some cases have had probative value as part of plaintiffs' evidence to establish a violation are:
>
> whether there is a significant lack of responsiveness on the part of elected officials to the particularized needs of the members of the minority group.
>
> whether the policy underlying the state or political subdivision's use of such voting qualification, prerequisite to voting, or standard, practice or procedure is tenuous.

does this by using illustrative elections from the jurisdiction to demonstrate that minorities and whites vote for different candidates, and the minority-preferred candidates consequently lose. Presidential elections, let alone the Obama victory, would not ordinarily be seen as typical or illustrative for most voting rights cases, the lion's share of which concern municipal or state legislative elections.[33] However, whether the 2008 election is offered as a counterexample in litigation or merely as a talking point concerning the racial polarization that section 2 is supposed to address in general, the data from it draw attention to some of the ongoing controversies in this area of the law.

In particular, the election has brought to the fore the oft-made arguments concerning the role of partisanship in measuring racial polarization and the definition of a minority candidate of choice.[34] Without getting too deep into the weeds of voting rights law, suffice it to say that the correlation between race and partisanship has posed some challenges to the *Gingles* framework.[35] The central question in this realm concerns how the law should respond to a situation in which a high correlation between race and partisanship makes it difficult for a plaintiff to prove that race, rather than party, better "explains" the voting behavior of different groups. In other words, if minority-preferred (usually Democratic) candidates lose because white Republicans tend to vote against them, does the partisan "explanation" for their loss immunize the districting plan from liability for the racially disparate impact it nevertheless presents?

For the *Gingles* plurality, the bivariate relationship between race and vote choice was the only relevant statistic. Justice Brennan's opinion stated:

> For purposes of § 2, the legal concept of racially polarized voting incorporates neither causation nor intent. It means simply that the race of voters correlates with the selection of a certain candidate or candidates; that is, it refers to the situation where different races (or minority language groups) vote in blocs for different candidates.[36]

In other words, because section 2 is about disparate impact, not intent, the "reason" why voters of one racial group might differ from another in their voting preferences is unimportant. Plaintiffs should not be

Gingles, 478 U.S. at 36–37 (quoting S. REP. NO. 97-417, at 28–29, *reprinted in* 1998 U.S.C.C.A.N. 177, 206–07).

[33] *See* Kristen Clarke, *The Obama Factor: The Impact of the 2008 Presidential Election on Future Voting Rights Act Litigation*, 3 HARV. L. & POL'Y REV. 59, 62 (2009).

[34] *See* Richard H. Pildes, *Is Voting-Rights Law Now at War with Itself? Social Science and Voting Rights in the 2000s*, 80 N.C. L. REV. 1517, 1522, 1529 (2002) (describing the significance of rising partisan competition in the South for purposes of voting rights law).

[35] *See generally id.*; Elizabeth M. Ryan, Note, *Causation or Correlation? The Impact of* LULAC v. Clements *on Section 2 Lawsuits in the Fifth Circuit*, 107 MICH. L. REV. 675 (2009).

[36] *Gingles*, 478 U.S. at 62 (plurality opinion).

forced to show racial animus, only that the voting behavior of different groups makes it more difficult for minority-preferred candidates to be elected. Indeed, as many commentators have argued, attitudes on issues concerning race have sometimes led voters to affiliate with particular parties.[37] Moreover, in a strict statistical sense, neither race nor party *causes* someone to vote for a particular candidate. Rather, at most, they represent group characteristics that might shed light on the reasons — racial identity or animus on the one hand, or ideological affinity and partisan loyalty on the other — why a voter might prefer one candidate over another. Even if multivariate analysis might demonstrate that partisanship is a more powerful predictor of candidate preferences than race, divergent voting behavior is still what prevents minorities under certain districting arrangements from having an "equal opportunity to elect" their preferred candidates.

Nevertheless, the view that a mere bivariate relationship between race and vote choice should suffice did not garner a majority of the Court, and the lower courts have been split as to whether a strong party-race correlation can defeat a claim of racial polarization. The Fifth Circuit sitting en banc in *League of United Latin American Citizens v. Clements*,[38] for example, held that the *Gingles* test is not satisfied "[w]hen the record indisputably proves that partisan affiliation, not race, best explains the divergent voting patterns among minority and white citizens."[39] The Fifth Circuit is not alone. One study found that "[c]ourts in nine judicial circuits now expressly or implicitly incorporate causation when they assess racial bloc voting."[40]

One way courts attempt to address this intractable race-party dynamic (as well as to resolve the related issue of who is a minority community's "candidate of choice") is to focus on elections in which minority candidates oppose white candidates.[41] If minority Democratic candidates tend to receive less of the white vote than white Democratic candidates, the argument goes, then race, rather than party, might better "explain" voting patterns. The *Gingles* plurality (and only the plurality) emphasized that "it is the *status* of the candidate as the *chosen representative of a particular racial group*, not the race of the candidate, that is important."[42] However, the lower courts have often considered elections that pit minority candidates against white

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[37] *See, e.g.*, Bernard Grofman & Lisa Handley, *1990s Issues in Voting Rights*, 65 MISS. L.J. 205, 222–30 (1995); Pamela S. Karlan & Daryl J. Levinson, *Why Voting is Different*, 84 CAL. L. REV. 1201, 1223–26 (1996) (discussing difficulties disaggregating race and politics).

[38] 999 F.2d 831 (5th Cir. 1993) (en banc).

[39] *Id.* at 850; *see also id.* at 863 ("Electoral losses that are attributable to partisan politics do not implicate the protections of § 2.").

[40] Katz et al., *supra* note 30, at 671.

[41] *See id.* at 665–68; Persily, *supra* note 11, at 221–23; Pildes, *supra* note 34, at 1526 n.22.

[42] Thornburg v. Gingles, 478 U.S. 30, 68 (1986) (plurality opinion).

candidates to be the "most probative" of legally significant racial bloc voting.[43]

We mention these gray areas in the law because the data we provide later in this Article allow us to grapple with and shed light on these controversies at a macro level. In particular, we pay close attention to the counterargument that party or ideology "explains" the gap in candidate preferences that exists between minorities and whites. Much of the story we tell is a familiar one that demonstrates the breakdown of the Democratic Party's monopoly in the South and the rise of the Republican Party among Southern whites. However, we also compare earlier results with those from the 2008 election, in which Barack Obama was, by any definition, the candidate of choice of African American voters.[44] In some states, this undisputed candidate of choice did worse among whites than did his predecessor who ran under much less favorable circumstances.[45]

At the same time, Obama's relative success in many other states may reveal the potential for minority candidates in those jurisdictions. In many states, all outside the South, Obama was able to win the white vote and therefore win the state.[46] In still others (ten states according to the exit polls, including North Carolina, Virginia, and Florida), he lost among whites, but minority voters put him over the top.[47] Finally, there are the states he lost, where he did not win a substantial share of the white vote and/or the minority population was not sizable enough for him to make up for that loss.[48] To use the parlance of section 2 to describe the geography of his victory: some states exhibited low rates of white bloc voting, and in others, despite high bloc voting, the minority community could still elect its candidate of choice.

Throughout our discussion of the data we refer to "racial differences in voting" or "racially differential voting patterns," in order to avoid the loaded jargon of polarization in section 2 jurisprudence. Accommodating the divergent notions of racial polarization discussed above,

[43] *See* Issacharoff, *supra* note 20, at 1855 n.111 (citing cases that discuss candidate race); Scott Yut, Comment, *Using Candidate Race To Define Minority-Preferred Candidates Under Section 2 of the Voting Rights Act*, 1995 U. CHI. LEGAL F. 571, 583–86 (describing Fifth and Seventh Circuit cases that discount races involving only white candidates).

[44] Because Obama did not win a majority of Hispanic votes in the Democratic primary, however, it is questionable whether he should be considered the Hispanic candidate of choice. Support from minorities in the general election ordinarily does not suffice to demonstrate a candidate is the choice of the minority community. *See LULAC*, 548 U.S. 399, 445–46 (2006) (opinion of Kennedy, J.) (suggesting that Martin Frost did not demonstrate that he was the candidate of choice of the African American community merely because the community preferred Frost over a Republican candidate).

[45] *See* Table 9.

[46] *Id.*

[47] *Id.*

[48] *Id.*

we present both bivariate correlations and multivariate regressions that attempt to control for partisanship and other factors that influence the vote. By comparing the 2008 election with its predecessors, moreover, we can discern changes in group-based voting behavior under the unique conditions when an African American candidate appears in the race. Whether we look at the simple correlations between race and vote choice or at multivariate analysis that controls for other demographic, partisan, or ideological variables, the results are the same. Race played a greater role in predicting vote choice in the 2008 presidential election than it did four years earlier, and whites in the covered states were less likely to vote for Obama than whites in the noncovered states.

B. The Coverage Formula for Section 5 of the VRA

Our principal goal in the data analysis here was to assess the differences in race-based voting patterns between the covered and noncovered jurisdictions under section 5 of the VRA. Jurisdictions are covered under section 5 if they employed a "test or device," such as a literacy test, and had voter turnout under 50% in the 1964, 1968, or 1972 elections.[49] The primary question involved in the constitutional challenge to section 5 that the Supreme Court considered last year was whether such jurisdictions continue to represent a greater threat to minority voting rights than noncovered jurisdictions.[50] Although presidential election returns themselves do not say anything of relevance to most of the core questions of minority voting rights, analyzing them serves two purposes for the current debate. First, it allows us to investigate the claim made by VRA opponents that the 2008 election represented a sea change in the preferences and behavior of voters from different racial groups in different regions.[51] Second, it allows us to assess the claims made as part of the 2006 reauthorization of the VRA concerning persistent racial polarization in the electorate of the covered jurisdictions.

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[49] 42 U.S.C. § 1973c (2006). In the 1975 amendments to the VRA, Congress expanded the definition of tests or devices to include the provision of English-only ballot materials in jurisdictions with large non-English-speaking populations. *See* CIVIL RIGHTS DIV., U.S. DEP'T OF JUSTICE, SECTION 4 OF THE VOTING RIGHTS ACT (July 25, 2008), *available at* http://www.justice.gov/crt/voting/misc/sec_4.php.

[50] *See NAMUDNO*, 129 S. Ct. 2504, 2512 (2009) ("The evil that § 5 is meant to address may no longer be concentrated in the jurisdictions singled out for preclearance. The statute's coverage formula is based on data that is now more than 35 years old, and there is considerable evidence that it fails to account for current political conditions.").

[51] *See* ABIGAIL THERNSTROM, VOTING RIGHTS AND WRONGS: THE ELUSIVE QUEST FOR RACIALLY FAIR ELECTIONS 200–01 (2009); Barnes, *supra* note 8; Liptak, *supra* note 8; Toobin, *supra* note 8.

Although this Article focuses on election returns, we should not pretend that such analysis represents the touchstone for evaluating whether Congress has exceeded its authority under the enforcement clauses of the Fourteenth or Fifteenth Amendments. In its consideration of previous versions of the VRA, the Court asked whether the legislation was a "rational means" of preventing or remedying violations of minority voting rights.[52] The legislative record supporting each incarnation of the VRA has focused on actual examples of racial discrimination, intimidation, and violation of minority voting rights, as well as data concerning minority voter turnout and officeholding.[53] However, the Court's decision in *City of Boerne v. Flores*[54] established that Congress's power to enforce the guarantees of the Fourteenth Amendment only extends to laws that were "congruen[t] and proportional[]" to the constitutional violations that the laws attempt to prevent or remedy.[55] Under this new standard, the question arises whether Congress must justify the coverage formula by distinguishing between covered and noncovered states in their relative rates of violation of minority voting rights.

The evidence of racial differences in voting patterns is useful and interesting (if not constitutionally determinative) for the current debate over the coverage formula for several reasons. First, it allows for a systematic comparison between covered and noncovered states along a dimension that should not be directly affected by the existence of section 5 itself. One problem confronting those who would gather data regarding the relative position of jurisdictions in their protection of minority voting rights is that section 5 deters and prevents covered jurisdictions from committing the kinds of constitutional violations that would constitute the best evidence for their selective coverage under the VRA.[56] In other words, if the VRA works as intended, the Department of Justice (DOJ) will prevent the emergence of discriminatory barriers to registration and voting, and no constitutional differences should appear between the election law regimes of the covered and noncovered jurisdictions.

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[52] South Carolina v. Katzenbach, 383 U.S. 301, 324 (1966).

[53] *See* Persily, *supra* note 11, at 192–207.

[54] 521 U.S. 507 (1997).

[55] *Id.* at 520.

[56] This catch-22 is what Chief Justice Roberts at oral argument in *NAMUDNO* described as the elephant whistle problem. "'You know, I have this whistle to keep away the elephants,' Roberts said rhetorically. 'Well, there are no elephants, so it must work.'" Jim Galloway, *With John Lewis in the Pews, Chief Justice John Roberts Compares Voting Rights Act to "An Elephant Whistle,"* ATLANTA J.-CONST., Apr. 29, 2009, http://blogs.ajc.com/political-insider-jim-galloway/2009/04/29/with-john-lewis-in-the-pews-chief-justice-john-roberts-compares-voting-rights-act-to-an-elephant-whistle.

Race-based voting patterns are largely exogenous to the legal regime — that is, they should not be directly affected by section 5 enforcement[57] — but might shed light on differential risks to minority voters were the section 5 regime to be removed. By themselves, these patterns do not point to unconstitutional state action, but they signal the relative potential for minority voters to elect their preferred candidates. Also, when candidate preferences coincide with racial group membership, there is greater risk that incumbent-protecting or partisan election-related behavior on the part of the legislature will have race-based effects. To put it concretely, when those who write election laws under such circumstances succumb to the tendency to enact regulations that benefit their electoral prospects, they enact laws with discriminatory effects. If blacks all vote Democrat and whites all vote Republican, for instance, an election law that seeks to perpetuate Republican control will often have discriminatory effects, even if it is not unconstitutional. The likelihood that partisan or even merely incumbent-entrenching behavior will have a disparate impact on voting rights is greater under conditions of race-based voting.

This last point poses obvious dangers as a constitutional justification for selective coverage of areas that experience racial differentiation in voting. These issues are reminiscent of the concerns related to the role of partisan voting in vote dilution litigation under section 2. If racially differential voting patterns on their own could justify singling out a jurisdiction for special treatment, then party-line voting could doom a jurisdiction to coverage until the very late date when parties and racial groups realign. Perhaps that should not make a constitutional difference: such risks of discriminatory state action fueled by partisan concerns either exist or do not exist, regardless of "cause." If Congress's decision to single out jurisdictions represents, at least in part, an assessment of the relative risks to minority voters in different places, then the mixed motives of those who may draft election laws do not bear on that risk assessment. Nevertheless, because constitutionally impermissible race-based discrimination requires intent — that is, discrimination "'because of,' not merely 'in spite of,'"[58] its race-

[57] It is possible, of course, that the existence of section 5 itself could diminish racial polarization in the electorate. Insofar as DOJ enforcement of section 5 has led to the creation of districts where minority candidates can be elected, perhaps white voters, over time, have become accustomed to and comfortable with minority-preferred candidates. Indeed, the higher rates of white support for minority incumbents, as opposed to minority candidates in open seats, might suggest precisely that dynamic. *See* Bernard Grofman, Lisa Handley & David Lublin, *Drawing Effective Minority Districts: A Conceptual Framework and Some Empirical Evidence*, 79 N.C. L. REV. 1383, 1394–98 (2001). However, even if this argument is valid, voting patterns, as opposed to voting laws, still will be less affected by the deterrence provided by the mere presence of section 5.

[58] Pers. Adm'r v. Feeney, 442 U.S. 256, 279 (1979).

based effects — partisan voting behavior that leads to partisan election laws does not necessarily constitute unconstitutional state action.[59]

At a more fundamental level, the relevance of the 2008 election to the VRA's constitutionality depends on whether one believes group-based voting behavior and election results, in general, are relevant to the constitutionality of the coverage formula. If the only relevant pieces of evidence to bolster the constitutionality of the coverage formula are, for this Court, instances of unconstitutional discrimination, then mere individual voting behavior does nothing to help the constitutionality of section 5. As Justice Thomas's separate opinion in *NAMUDNO* maintained, "[R]acially polarized voting is not evidence of unconstitutional discrimination [and] is not state action . . . ."[60] Although voting may occur in a state-structured and regulated environment (that is, the polling place), the choices made are personal ones to the voter, not efforts by the state. Even if one might view the state as enabling discrimination against either minority voters who get outvoted or their preferred candidates who lose, an individual's or group's vote choice, even based on racial animus, does not itself violate the Constitution. If Congress's authority under the enforcement clauses of the Fourteenth and Fifteenth Amendments, for this Court, extends only to remedying or preventing actual constitutional violations (an open question to be sure, especially under the Fifteenth Amendment), then persistent polarized voting by itself does not constitute a sufficient record for an exercise of congressional power in this realm.

At the same time, a lack of polarized voting does not speak to whether racial minorities face increased obstacles or unconstitutional conditions at the polls. Even if the same percentages of whites and African Americans across the country vote for the same candidates, for instance, the relative prevalence of discriminatory voting laws in some areas could still justify a geographically targeted voting rights law. If, in the 2008 election, candidate preferences were completely uncorrelated with race, it still could be possible that minority voters in certain jurisdictions faced discriminatory barriers to voting.

For these reasons, we view the election data we analyze for the remainder of this Article as primarily a response to the argument that lessened racial polarization undermines the justification for the cover-

[59] We should emphasize that, for us, the questions surrounding the constitutionality of the VRA are easy ones. *See* Persily, *supra* note 12, at 260–61 (arguing that the reauthorized VRA should be considered constitutional according to a lowered standard for exercises of congressional power to protect against racial discrimination in voting rights).

[60] *NAMUDNO*, 129 S. Ct. 2504, 2526 (2009) (Thomas, J., concurring in the judgment in part and dissenting in part) (citation omitted); *see also* United States v. Morrison, 529 U.S. 598, 621 (2000) (striking down the Violence Against Women Act as exceeding Congress's Enforcement Clause authority in part because it regulated private behavior).

age formula,[61] rather than as support for the suggestion that the data, on their own, justify its constitutionality.[62] For those advocates who would recraft the coverage formula to pay particular attention to the jurisdictions with higher rates of racial differentiation in voting, the data that follow indicate the jurisdictions of concern. There can be no doubt that race-based patterns in voting behavior are greater in the currently covered jurisdictions — on average.[63] Yet the categories of currently covered and noncovered are not coterminous with those jurisdictions with the highest rates of race-based voting.[64] If voting patterns were to form the exclusive justification for coverage — something no one has suggested — then the list of covered states would be somewhat different.

II. RACIAL DIFFERENCES IN VOTING PRIOR TO 2008

Racial and regional differentiation in presidential vote choice are familiar characteristics of American electoral politics. The two parties and their nominees have always been able to rely on some groups and regions more than others, even if allegiances have changed (sometimes radically) over time. Some of these patterns of racial and regional differentiation in vote choice held true for the 2008 election, while others were disrupted.

The underlying story concerning these patterns is familiar.[65] The flight of African Americans away from the GOP, with which they had largely affiliated since the end of slavery, began to occur during Franklin Roosevelt's presidency.[66] While Roosevelt won only 23% of the black vote in 1932, the popularity of the New Deal among blacks led to a realignment such that by 1948 Truman would win 70% of the black vote.[67] Although Southern blacks may have preferred the largely absent Republican Party, most were prevented from voting altogeth-

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[61] *See* THERNSTROM, *supra* note 51, at 200–02.

[62] *See* Clarke, *supra* note 33, at 84–85.

[63] *See* Table 5.

[64] *See* Table 9.

[65] *See generally* NELSON W. POLSBY, HOW CONGRESS EVOLVES (2004) (describing the rise of the Republican party in the South due to Northern migration and conversion of Dixiecrats); Lisa Handley & Bernard Grofman, *The Impact of the Voting Rights Act on Minority Representation*, *in* QUIET REVOLUTION IN THE SOUTH 335, 335–77 (Chandler Davidson & Bernard Grofman eds., 1994) (detailing the effect of the VRA on white and black voting behavior).

[66] *See* ANGUS CAMPBELL, PHILIP E. CONVERSE, WARREN E. MILLER & DONALD E. STOKES, THE AMERICAN VOTER: AN ABRIDGEMENT 92–93 (1964); NANCY J. WEISS, FAREWELL TO THE PARTY OF LINCOLN: BLACK POLITICS IN THE AGE OF FDR 180 (1983).

[67] *See* David Greenberg, *The Party of Lincoln . . .*, SLATE, Aug. 10, 2000, http://slate.msn.com/id/87868.

er.[68] The legacy of Lincoln held sway over Southern whites, which made the South solid for Democratic nominees through the 1950s.[69]

Since Lyndon Johnson's landslide victory in 1964 (followed by the passage of the VRA in 1965), the Democrats have not won a majority of the white vote nationally in a presidential election, due in large part to their losses among Southern whites.[70] Republican nominees have consistently won a majority (or plurality) of the white vote.[71] The Democratic nominee has repeatedly won a majority of the vote from racial minorities — overwhelming majorities among African Americans and slimmer, but consistent majorities among Hispanics.[72] The magnitude of these racial differences in vote choice varies according to region. In particular, the share of whites in the covered jurisdictions, especially in the South, who have voted for Democratic nominees has been smaller than the share outside the covered jurisdictions or the South.[73] As a result, given the relative consistency of the minority vote across the nation, the gap between whites and minorities is more substantial in the covered states.

A. Presidential Election Exit Polls, 1984–2004

Since the reauthorization of section 5 of the VRA in 1982,[74] these patterns of vote choice at the presidential level have been relatively stable. Table 1 presents the average share of the two-party vote received by Democratic candidates according to exit polls from 1984 through 2004 as broken down by race, party, and covered status. The

[68] *See* V.O. KEY, JR., SOUTHERN POLITICS IN STATE AND NATION 517 (Univ. of Tenn. Press 1984) (1949) ("So few have been Negro voters in the South that to estimate their number seems futile.").

[69] *See* EARL BLACK & MERLE BLACK, THE VITAL SOUTH 4–5 (1992).

[70] *See id.* at 149–58 (discussing Goldwater's strategy of appealing to Southern whites disaffected with the Democratic party).

[71] Because of the Perot candidacy in 1992 and 1996, no party won a majority of the white vote. Exit polls began in 1976. Data on voting patterns of racial groups before then are available through the American National Election Studies (ANES). Using the Cumulative Data File, 1948–2004, which combines all of the ANES surveys, we calculate the percentage of whites voting Democratic in each of the presidential elections from 1948 to 2004. UNIV. OF MICH., CTR. FOR POLITICAL STUDIES, THE NATIONAL ELECTION STUDIES, 1948–2004 ANES CUMULATIVE DATA FILE (2005), http://www.electionstudies.org/studypages/cdf/cdf.htm. According to the ANES data the percentage of whites who voted Democratic in each election is estimated to be: 51% in 1948, 40% in 1952, 39% in 1956, 48% in 1960, 64% in 1964, 36% in 1968, 30% in 1972, 46% in 1976, 33% in 1980, 35% in 1984, 39% in 1988, 41% in 1992, 46% in 1996, 46% in 2000, and 41% in 2004. According to the ANES data, the percentage of minorities (all combined) who voted Democratic in each election is estimated to be: 73% in 1948, 62% in 1952, 66% in 1956, 73% in 1960, 99% in 1964, 91% in 1968, 77% in 1972, 80% in 1976, 80% in 1980, 71% in 1984, 78% in 1988, 74% in 1992, 80% in 1996, 69% in 2000, and 70% in 2004.

[72] *See id.*; Table 1; Figure A.

[73] *See* Table 1.

[74] Voting Rights Act Amendments of 1982, Pub. L. No. 97-205, § 2, 96 Stat. 131 (codified as amended at 42 U.S.C. § 1973 (2006)).

TABLE 1. THE RACIAL GAP IN VOTING FOR DEMOCRATIC NOMINEE, PRESIDENTIAL EXIT POLLS, 1984-2004[75]

Group	Covered	Noncovered + Partially Covered	Nation
White	28	42	39
Black	84	84	84
Latino	61	64	63
Whites			
Democrats	72	79	78
Republicans	4	9	8
Independents	28	42	40
Difference			
Black-white	56	42	45
Latino-white	33	22	24

[75] These data were gathered from national exit polls archived at the Inter-University Consortium for Political and Social Research (ICPSR), https://www.icpsr.umich.edu/icpsrweb/ICPSR/access/series.jsp (last visited Feb. 27, 2010). All calculations were performed using sample weights provided by the exit poll in the relevant file. In all ICPSR files, the weight variables are labeled WGT. The exit poll results are weighted to reflect the complexity of the sampling design and to take into account the different probabilities of selecting a precinct and of selecting a voter within each precinct. The weights are defined such that the exit poll results equal the final tabulated vote within geographic regions of the states or nation. Calculations were made for each state using the within-state weights provided by the exit polls. Next, aggregate calculations were made for VRA and non-VRA regions, weighting each state by the population of interest (i.e. Whites, Blacks, Hispanics, White Democrats, White Republicans, and White Independents) residing in that state.

states with some covered counties or municipalities do not differ in any meaningful way from those that are totally noncovered, so we combine the two. In the noncovered states with covered municipalities, only a minority of the population — in most such states, a very small minority — is actually covered. North Carolina is the partially covered state with the greatest share (36%) of its population covered. New York is second with 28% of its population covered. In all other partially covered states, the share of the state's population that is covered is negligible. We also adopt the DOJ's practice of including Virginia as a fully covered state, even though several of its municipalities (amounting to a very small share of its total population) have bailed out from coverage.[76]

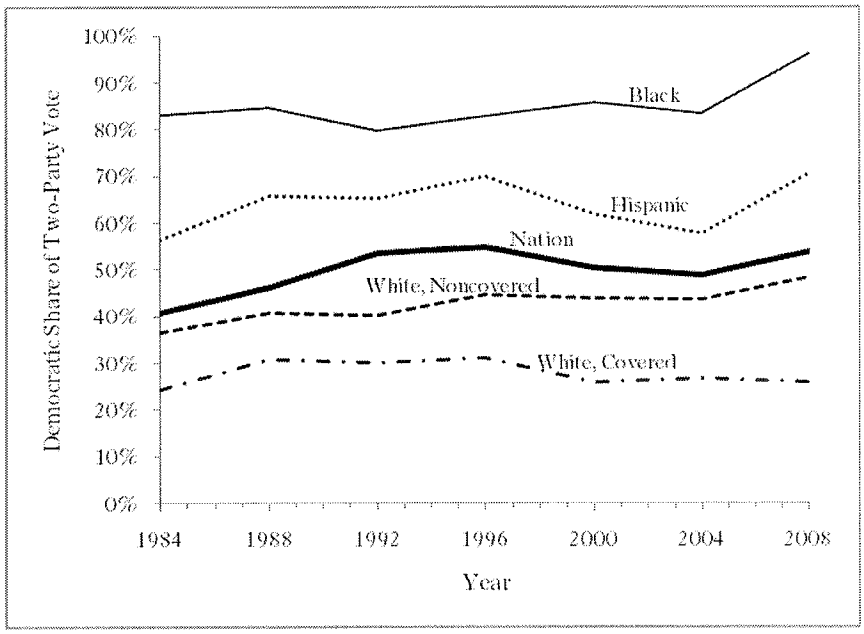
As noted above, whites in the covered jurisdictions voted distinctly more Republican than whites in the noncovered jurisdictions. Only 28%, on average, said they voted for the Democratic nominee — fourteen percentage points lower than their counterparts in the noncovered jurisdictions, where 42% of whites on average reported voting for Democratic nominees. This is thirty-three percentage points lower than Democratic nominees' average vote share among Latinos (61%) and fifty-six percentage points lower than the average among African Americans (84%) in the covered jurisdictions. Regardless of whether they live in covered or noncovered jurisdictions, racial minorities, in contrast, do not seem to differ substantially in the share that report voting for Democratic nominees.

The regional differences between whites occur among all partisan subgroups. In the covered states, whites of every partisan affiliation (or nonaffiliation) were less likely than whites in the noncovered states to vote for the Democratic nominee. The difference was most stark among Independents, who exhibit a fourteen percentage point gap (42% versus 28% support in the covered areas). However, the gap is seven percentage points among white Democrats and five percentage points among white Republicans between the covered and noncovered states. In sum, differences in whites' voting preferences across covered and noncovered areas cannot be attributed wholly to party. Republican identifiers, Democratic identifiers, and especially Independent identifiers in covered jurisdictions vote for Republican candidates at higher rates than do their counterparts in noncovered jurisdictions. Of course, it is well known that Southern white Democrats have been voting for Republican presidential candidates for many decades now.[77] Whether these voting patterns might be attributable to their ideologi-

[76] *See* CIVIL RIGHTS DIV., *supra* note 10 (listing as covered states: Alabama, Alaska, Arizona, Georgia, Louisiana, Mississippi, South Carolina, Texas, and Virginia).

[77] *See* sources cited *supra* note 65.

FIGURE A. DEMOCRATIC CANDIDATE'S SHARE OF THE TWO-PARTY VOTE, NATIONAL EXIT POLLS, 1984-2008



cal conservatism, not accurately or completely gauged by their self-identification with a party, is a topic we address later in the Article. At least in the 2008 election, it appears that some variable not captured by either partisanship or ideology still accounts for the differences in vote choice between the covered and noncovered states.

The race-based patterns in presidential vote choice have been remarkably stable over the past two decades. Figure A graphically depicts the share of the two-party vote received by Democratic nominees, broken down by racial group, from 1984 to 2008. Blacks have been most supportive of Democratic candidates, followed by Hispanics. Republicans won a majority or plurality of the white vote in every election from 1984 to 2008. That pattern holds true in covered and noncovered jurisdictions, but the whites in covered jurisdictions vote more heavily Republican than those in noncovered jurisdictions. The gap between whites in the noncovered and covered states has varied between 10 percentage points in 1988 and 22 points in 2008. In 1996 Bill Clinton received 31% of the white vote in the covered states, the highest received by a Democratic nominee during this period. Walter Mondale received the lowest, with 24% of the vote in 1984. For the most part, it appears Democrats have been doing steadily better among whites in the noncovered states over time, while support among whites in the covered states has remained largely flat.

B. The 2004 Election

We explore in greater detail the results of the 2004 election because its close proximity to the 2008 election allows it to serve as a benchmark for comparison. With a few exceptions, the 2004 results are largely similar to the averages from 1984 to 2004. We present both the cross-tabulations from the exit polls and regressions that analyze aggregated election returns. The data, regardless of presentation, support the same result. As Table 2 depicts, the 2004 exit polls reveal that John Kerry did somewhat worse than previous Democratic nominees among Hispanics (by about three percentage points) and did somewhat better than previous Democratic nominees among whites and African Americans (by about two to three percentage points). Relative to the average, he lost among whites in the covered jurisdictions but made up for it among whites in the noncovered jurisdictions. He lost ground among Hispanics regardless of coverage status, but his loss was more pronounced in the covered states. Compared to the partisan structure of the white vote received by previous Democratic nominees, he did substantially better among white Democrats and Independents and slightly worse among white Republicans. This was true for both the covered and noncovered states.

In terms of their reported vote in the 2004 election, only Hispanics and whites exhibited significant differences based on whether they

TABLE 2. RACIAL GAP IN 2004 PRESIDENTIAL ELECTION[78]

Group	Covered	Noncovered + Partially Covered	Nation
White	26	44	41
Black	88	87	87
Latino	50	63	60
Whites			
Democrats	83	86	86
Republicans	3	7	6
Independents	34	51	48
Difference			
Black-white	61	43	46
Latino-white	24	19	18

TABLE 3. RELATIONSHIP BETWEEN COUNTY RACIAL MAKEUP AND PRESIDENTIAL VOTE, 2004[79]

	Covered	Noncovered
Black and Hispanic percentage of county population	0.534 (0.021)**	0.374 (0.013)**
Constant	0.236 (0.009)**	0.421 (0.004)**
N	860	2254
R-squared	0.42	0.26

*p<0.05, **p<0.01. Standard errors in parentheses.

[78] The data for this table are also available at ICPSR. INTER-UNIV. CONSORTIUM FOR POLITICAL AND SOC. RESEARCH, NATIONAL ELECTION POOL GENERAL ELECTION EXIT POLLS, 2004, http://dx.doi.org/10.3886/ICPSR04181 (last visited Feb. 27, 2010). Small discrepancies in the "Difference" calculations are due to rounding.

[79] Election returns were gathered by the authors from official state election returns. Data about the racial composition of counties in 2006 were taken from U.S. Census Bureau estimates for 2006. *See* U.S. CENSUS BUREAU, STATE & COUNTY QUICK FACTS, http://quickfacts.census.gov/qfd/index.html (last visited Feb. 27, 2010).

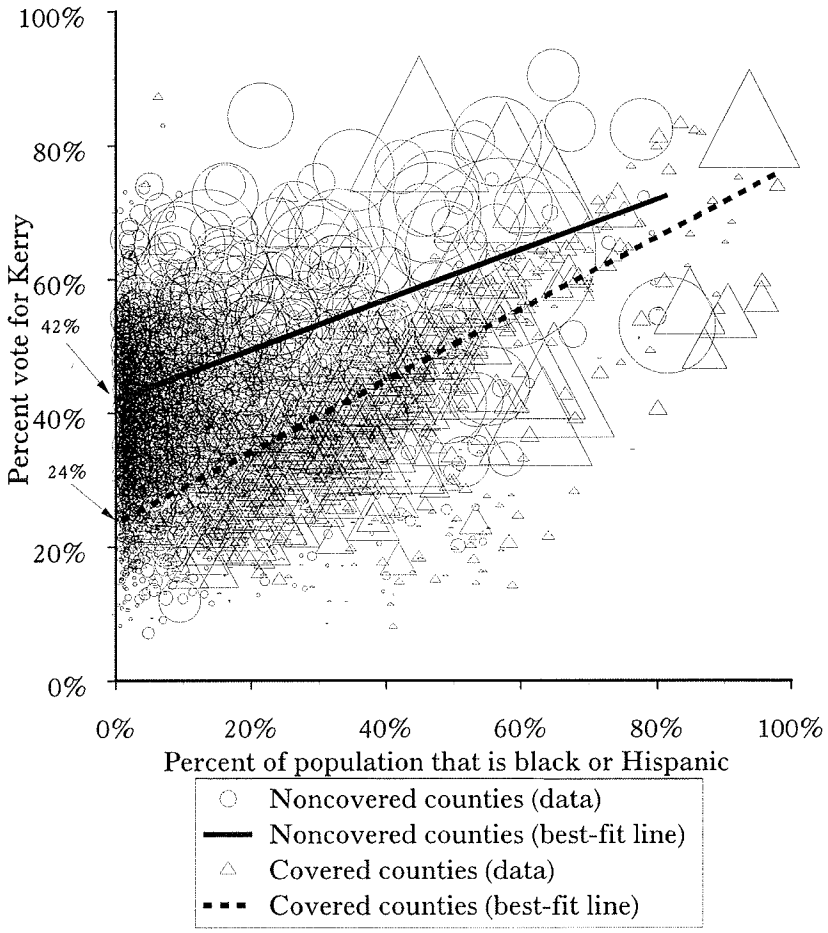
lived in the covered states. Twenty-six percent of whites in the covered states reported voting for Kerry, but 44% of whites in the noncovered states said they did so. The 2004 election also demonstrated a remarkable lack of cohesion among Hispanics, especially in the covered states. Only half of Hispanics in the covered states reported voting for Kerry, while 63% in the noncovered states did so. This lack of cohesion was a significant departure from Hispanic voting patterns between 1984 and 2004, where, on average, the covered and noncovered gap for Hispanics was only three percentage points. African Americans, in contrast, voted about the same, regardless of coverage status: 88% voted for Kerry in the covered states and 87% in the noncovered states.

The aggregated returns from the 2004 election are consistent with the exit poll data. Table 3 and Figure B present in different formats the election results by county, according to that county's racial makeup. Presenting the data in this way allows us to account for the covered counties in noncovered states, something we cannot do reliably with the state-based exit polls. Noncovered counties that contain some covered municipalities (as in New Hampshire) are considered not covered because a small percentage of the population of the county resides in the covered municipality. Figure B plots the relationship between a county's black plus Hispanic percentage and the share of the vote John Kerry received. The size of the triangles or circles in Figure B corresponds to the total voter turnout in the county, with triangles referring to counties covered by the VRA and circles referring to counties not covered by the VRA. The solid line is the best-fit regression line for the noncovered counties, and the dashed line is the best-fit regression line for the covered counties. Table 3 presents the data that are graphically expressed by those regression lines.

Figure B clearly (and unsurprisingly) demonstrates the positive relationship between a county's percentage of racial minorities and the share of the vote received by John Kerry. The two statistics of note are the intercepts with the y axis and the slope of the lines. The y-intercept denotes the share of voters in a county without blacks or Hispanics who tended to vote for Kerry. It is helpful in isolating the expected share of the white vote that the candidate received. The data suggest that, in the noncovered counties, John Kerry received 42% of the white vote, whereas he received only 24% of the white vote in the covered jurisdictions. The results are quite close to those found in the exit polls.

As is also clear from the data, the line for the covered counties is steeper than that for the noncovered counties. The slope of the line for the covered jurisdictions is 0.534 and for the noncovered jurisdictions is 0.374. Moreover, the R-squared value is higher as well for the covered jurisdictions (0.42 compared to 0.26). This suggests that Kerry's vote share in covered counties is better explained by its minority per-

FIGURE B. RELATIONSHIP BETWEEN 2004 PRESIDENTIAL VOTE AND NONWHITE POPULATION SHARE, COVERED AND NONCOVERED COUNTIES[80]



[80] Sizes of data tokens are proportionate to the number of voters turning out in the county. See *supra* note 79 for an explanation of the data used in Figure B as well as in Table 3. Regression lines are based on estimates reported in Table 3.

centage than is the case in the noncovered jurisdictions. For each additional percent of black or Hispanic population in a covered county the Kerry vote share in the covered counties increased by 0.53 percentage points, and in the noncovered counties it increased by 0.37 percentage points.

The data from the 2004 election, as with the aggregated data since 1984, point to the regional differences in the relationship between race and vote choice. Of course, the gap between whites and racial minorities is due both to the high share of the minority population willing to vote for the nominee and to the relatively lower share of whites willing to vote for him. An increase in racial differences can occur because of both a decrease in the white vote share and an increase in the vote share of racial minorities.

III. RACE AND VOTE CHOICE IN THE 2008 ELECTION

Barack Obama's victory in 2008 proved a very simple fact — a black candidate can win in the majority-white constituency that is the national presidential electorate. Some viewed his election as posing a challenge, therefore, to the philosophical and perhaps constitutional foundations of the Voting Rights Act.[81] If his election indicates decreasing racial polarization in the electorate, it might lower the prospects for success of potential plaintiffs in section 2 vote dilution cases. Moreover, if Obama's relative success among whites was uniform across the country, the 2008 election might bolster the arguments of critics concerning the irrationality or unconstitutionality of the geographically targeted coverage formula of section 5. We find, however, that the magnitude of race-based differences in voting preferences increased across the nation in the 2008 election, largely due to the increase in cohesive support of racial minorities for Obama. Obama did better than Kerry among whites in the noncovered states, but about the same as Kerry among whites in the covered states. Even within these two classes of jurisdictions, there is considerable diversity among the voting preferences of whites. However, controlling for demographics, partisanship, and ideology explains the differences between whites in the covered and noncovered jurisdictions in 2004, but not in 2008. In other words, regardless of whether one sees racial polarization in the simple correlation between race and vote choice (as did the plurality in *Gingles*) or would require a more nuanced analysis that controls for other potential explanations for racial divergence in voting (as have several circuits), states covered under the Voting Rights Act gen-

[81] *See, e.g.*, Thernstrom & Thernstrom, *supra* note 4; George Will, *Revise the Voting Rights Act*, REAL CLEAR POLITICS, Jan. 19, 2009, http://www.realclearpolitics.com/articles/2009/01/revise_the_voting_rights_act.html.

erally exhibit higher rates of racial polarization in presidential voting patterns.

In one very obvious way, the 2008 election departed from the dominant pattern of presidential elections since 1968: the Democratic nominee won. Both Presidents Carter and Clinton won during that period as well, but Republicans had won seven of the previous ten presi-presidential contests, usually by large margins.[82] Republican victories almost always included wins of the South and the white vote nationwide by a substantial margin.[83] They always lost the black and Hispanic vote, but their share of the minority vote varied by close to twenty percentage points depending on the election.[84]

In the immediate aftermath of the 2008 election, the most dramatic developments appeared to be the "new" states Obama won: for example, Virginia, North Carolina, Indiana, and Colorado. One could reasonably infer from victories in that diverse group of states that the Democrats had made inroads into previously Republican strongholds in the South and elsewhere, particularly among white voters. A rising, nationally uniform pro-Democratic (or anti-Republican) tide, it would seem, lifted Obama to victory in certain states that may have not appeared winnable in 2004 when the conditions and candidates seemed to favor the Republicans.

Once the exit poll and other survey data became available, however, the picture of the 2008 electorate appeared more complicated. Obama did better in some states but worse in others, as compared to John Kerry four years earlier. Moreover, the change in the composition of the electorate seemed as much, if not more, responsible for Obama's victory as the conversion of Bush voters. Higher black and Hispanic turnout, coupled with relatively lower white turnout, plus Obama's increase over 2004 in his vote share among minorities, were key to his victory.[85]

Both of these features of the 2008 election are relevant to questions underlying the VRA. The uneven geographic distribution of white supporters of Obama highlights where racial gaps in voting patterns might be narrowing and where they might be growing. Even conceding the uniqueness of presidential elections and the Obama candidacy, the changes between 2004 and 2008 may highlight areas of changing racial polarization affecting potential plaintiffs in section 2 lawsuits. Similarly, the high turnout of minorities and the changing composition

[82] Six of ten if one counts the 2000 presidential election as a Democratic win, given Al Gore's win of the popular vote.

[83] *See* Figure A; Dave Leip, Dave Leip's Atlas of U.S. Presidential Elections, http://uselectionatlas.org/RESULTS (last visited Feb. 27, 2010).

[84] *See* Figure A.

[85] *See* Tables 4 & 5.

of the electorate may hint at a future where, even in some racially polarized environments, minorities' increasing share of the electorate can counteract any tendency among whites to vote against minorities' preferred candidates.

A. National Results

Barack Obama won the 2008 general election because he won a larger share of both the white vote and the minority vote than John Kerry did four years earlier. In addition, the composition of the electorate differed from four years earlier, as racial minorities comprised a larger percentage of the voters who turned out. This combination of increased turnout and vote share — which was not constant across regions, states or groups — propelled Obama to victory.

The 2008 electorate was less white and more Democratic than it was in 2004, as Table 4 demonstrates. Ten million more people voted in 2008 than in 2004 (132.6 million in 2008 and 122.5 million in 2004).[86] However, the white share of the electorate decreased from 78% to 74%, while the African American share increased 2 points from 11% to 13%, and the Hispanic share increased from 8% to 9%. Among whites, Republicans dropped off dramatically — 4 points down to 29% of the 2008 electorate. White Democrats remained constant at 24%, while white Independents increased 1 point to become 22% of the electorate. In other words, the increase in the Democratic share of the electorate was due almost completely to increased turnout among minorities, and the decrease in the Republican share of the electorate was due to the drop off of whites.

Comparing the 2008 election exit polls to those from the 2004 election allows us to get a sense of what made the difference for the Democratic nominee. Obama received ten million more votes than Kerry. Approximately 70% of Obama's gain in votes over Kerry can be attributed to black and Hispanic voters. Obama received 4.3 million more votes from African Americans than did Kerry. Total turnout among blacks rose from 2004 to 2008, and they voted more solidly for the Democrats in 2008 than they did in 2004. Obama won 96% of the black vote (a 9 point increase from 2004). He also did markedly better among Hispanics than Kerry. Obama received 2.7 million more votes from Hispanics, from whom he won 70% of the vote. Kerry, by contrast, won only 59% of the Hispanic vote in 2004. Whites accounted for 3 million additional votes for the Democrats in 2008. In other words, although Obama received 3 million more white votes than Kerry (which translates into a three percentage point increase among

[86] *See* Michael McDonald, United States Elections Project, Voter Turnout, http://elections.gmu.edu/voter_turnout.htm (last visited Feb. 27, 2010).

TABLE 4. CHANGE IN THE COMPOSITION OF THE ELECTORATE 2004–2008[87]

Change in Racial Composition of the Electorate									
Race	Covered			Noncovered			Nation		
	2008	2004	diff	2008	2004	diff	2008	2004	diff
White	68	69	-1*	75	80	-5*	74	78	-4*
Black	21	19	2*	11	9	2*	13	11	2*
Hisp.	10	9	1*	9	7	2*	9	8	1*
Asian	1	1	0	2	2	0	2	2	0
Change in Partisan Composition of the Electorate									
Party	Covered			Noncovered			Nation		
	2008	2004	diff	2008	2004	diff	2008	2004	diff
Dem.	36	34	2*	40	38	2*	39	37	2*
Rep.	37	43	-6*	31	36	-5*	32	37	-5*
Ind./Other	27	23	4*	29	27	2*	29	26	3*
Whites by Party, as a Percentage of Entire Electorate									
Party	Covered			Noncovered			Nation		
	2008	2004	diff	2008	2004	diff	2008	2004	diff
Dem.	13	15	-2*	26	26	0	24	24	0
Rep.	33	38	-5*	27	32	-5*	29	33	-4*
Ind./Other	20	17	3*	23	22	1*	22	12	10*

* $p<0.01$.

[87] Exit poll data for 2004 come from the ICPSR, *supra* note 78. Exit poll data for individual states for 2008 come from CNN. CNN, ELECTION CENTER 2008, LOCAL EXIT POLLS, http://www.cnn.com/ELECTION/2008/results/polls/#ALPoop1 (last visited Feb. 27, 2010).

whites from 41% to 44%), most of the difference between the 2004 and 2008 elections can be explained by minority votes. Obama did better than Kerry in both total votes from minorities and percent of votes from minorities.

On the Republican side, McCain actually lost ground among minority voters compared to Bush. Four percent of black voters chose McCain in 2008, compared with 13% who chose Bush in 2004. Those figures imply that Bush received approximately 1.5 million votes and McCain just under 700,000 votes from blacks.[88] The total number of blacks who voted for the Republican standard-bearer actually fell from 2004 to 2008. A similar drop occurred among Hispanics. Bush won 40% of the Hispanic vote, and McCain captured just 30%. These figures suggest that Bush received 4.2 million votes from Hispanics, while McCain's Hispanic vote dropped to 3.7 million.[89] As the minority vote grew from 2004 to 2008, Republicans lost support in these communities not just as a percent of the total vote, but also in absolute numbers of minority votes.

B. The Section 5 Coverage Formula and the Results of the 2008 Election

Although rates of minority support for Obama were largely constant across the country, white support varied greatly by state and region. As a result, the size of the racial gap in support for Obama varied considerably as well. For the most part, but with some notable exceptions, white support was lower and the racial gap in Obama support was greater in states covered by the VRA. Indeed, in several states in the Deep South, Obama actually did worse among whites than Kerry. However, owing to increases in turnout by minorities and a decreased white share of the electorate, Obama did worse than Kerry in total share of the vote in only three states: Louisiana, Arkansas, and Tennessee.

Table 5 presents the exit poll data describing Obama's vote share in the covered and noncovered states and comparing it to 2004. One of the most striking statistics is the zero in the added vote share among whites in the covered jurisdictions between 2004 and 2008. Obama,

[88] These numbers were calculated from exit poll numbers reporting percentages of the electorate that were of each ethnic group in 2004 and 2008, *see* Table 4 & n.87, and total numbers of voters in 2004 and 2008 as reported by the Federal Election Commission, *see* FED. ELECTION COMM'N, FEDERAL ELECTIONS 2008: ELECTION RESULTS FOR THE U.S. PRESIDENT, THE U.S. SENATE AND THE U.S. HOUSE OF REPRESENTATIVES (2009), *available at* http://www.fec.gov/pubrec/fe2008/federalelections2008.shtml; FED. ELECTION COMM'N, FEDERAL ELECTIONS 2004: ELECTION RESULTS FOR U.S. PRESIDENT, THE U.S. SENATE AND THE U.S. HOUSE OF REPRESENTATIVES (2005), *available at* http://www.fec.gov/pubrec/fe2004/federalelections2004.pdf.

[89] *See* sources cited *supra* note 88.

like Kerry, won only 26% of the white vote in the covered states. For some, this is remarkable because a black candidate did as well among whites in the covered states as a white Democrat four years earlier. Even as recently as 2004, almost no one would have predicted that a black presidential candidate might do as well as the most recent white candidate in the covered states.

The context of the 2008 election, however, was not the same as 2004, and, viewed in that light, Obama's failure to improve over Kerry's margin among whites in covered jurisdictions stands out. The economic collapse, the historic unpopularity of a sitting Republican president, and an enormous fundraising advantage provided a wind at the Democrats' back.[90] Indeed, outside of the covered states, Obama did much better. With the exception of whites in the covered states, Obama made statistically significant gains among all racial groups, regardless of coverage status. The added vote share was most pronounced among minorities, but he also gained 4 percentage points among whites in the noncovered states, where he won 48% of the white vote.

Partisan affiliation does not account fully for the differences between whites in the covered states and either their predecessors in 2004 or whites in the noncovered states. Obama received 75% of the white Democratic vote in the covered states[91] — 7 percentage points less than Kerry, while remaining about constant among white Democrats in the noncovered states, where he won 85% of the white Democratic vote. For each partisan grouping of whites Obama did better in the noncovered states than in the covered states: 10 points better among white Democrats (85% versus 75%), 6 points better among white Republicans (10% versus 4%), and most significantly, 19 points better among white Independents (50% compared to 31%).

Not only did the covered jurisdictions differ from the noncovered jurisdictions in their levels of white support for Obama, but they also became more different in 2008 as the racial gap in the covered jurisdictions grew. The racial gap in voting preferences — that is, the percent of the white vote received by Obama minus the percent of the

[90] As Bullock and Gaddie show, in the covered states, white Democratic candidates in down-ballot races performed about 10 percentage points better than Obama, further suggesting that Obama performed worse than an average Democrat would have. *See* CHARLES S. BULLOCK III & RONALD KEITH GADDIE, THE TRIUMPH OF VOTING RIGHTS IN THE SOUTH 364 (2009).

[91] To arrive at the party-by-race totals for the covered and noncovered states requires weighting the state-based exit poll results by the share of each racial and partisan grouping. Because data as to the racial composition of each state's party membership are not available, we rely on the exit polls' assessments before combining states into the covered and noncovered groupings. Alternative weighting regimes, such as weighting simply according to census figures as to a state's racial composition, produce results with different magnitudes, although the basic story we tell here remains unaffected.

TABLE 5. RACIAL GAP IN PRESIDENTIAL VOTING PREFERENCES, 2008 EXIT POLLS[92]

	Covered States		Noncovered States		Nation	
Group	2008 (%)	Change from 2004	2008 (%)	Change from 2004	2008 (%)	Change from 2004
White	26	0	48	4**	44	3**
Black	97	9**	96	9**	96	9**
Latino	67	16**	72	9**	70	11**
Whites						
Democrats	75	-7**	85	0	84	-1**
Republicans	4	1**	10	4**	9	3**
Independents	31	-3	50	-2	47	-2
Difference						
Black-white	71	9**	48	5**	52	6**
Latino-white	41	16**	24	5**	26	8**

* p<0.05, ** p<0.01. Small differences in the numbers in Tables 2 and 5 are due to rounding.

[92] For an explanation of the ICPSR data used in Table 5, see *supra* note 87.

minority vote — increased nationally in 2008. The racial gap between blacks and whites grew 6 points (from 46 percentage points to 52), and the gap between Hispanics and whites grew 8 points (from 18 to 26 percentage points). However, the large racial gaps in preferences in the covered states grew even further in 2008, and grew more than the racial gap in the noncovered states. The 71 percentage point gap that separates blacks and whites in the covered states represents a 9 percentage point increase from 2004. By comparison, the noncovered states experienced a growth in the black-white gap of only 5 percentage points, such that 48 percentage points separate blacks and whites in the noncovered states. As a whole, the covered states became more different, not less, from the noncovered states with respect to the gap in voting preferences between the races.

Analysis of the aggregated election returns confirms the findings from the exit polls. Figure C and Tables 6, 7, and 8 present county-based results from the 2008 election to examine the relationship between a county's combined black and Hispanic share of the population and the share of the vote Obama received in the county.[93] As in our discussion above, the key statistics are the slope and intercept of the regression lines. The *y*-intercept for the covered counties remains about the same as it was in 2004, at 24%, signifying that about 24% of whites in the covered counties voted for Obama. The intercept for the regression line for the noncovered counties rises 4 percentage points from 2004 — from 42% to 46%. The line becomes steeper as well, and particularly so for the covered counties. The slope for 2008 for the covered counties is 0.635, compared to 0.415 for the noncovered counties — both slopes are higher than they are in Figure B.[94] The greater steepness of the regression line for the covered counties comes both from the "push" of Obama's increased vote share among minorities (raising the right side of the line) as well as the anchor of his share among whites, which keeps the intercept about where it was in 2004. As Table 6 depicts, the R-squared also increases substantially, suggesting that race became a better explanation for the presidential results. Almost half of the variance in presidential voting results between covered counties can be explained by their racial makeup.[95]

[93] Because county-based results for Alaska are not available, we treat Alaska as one large county. Also, noncovered counties with covered municipalities are considered noncovered, given that such a small share of the population of the county is covered.

[94] Presidential vote data were gathered by the authors using official election return reports published by the states. The source of the county racial composition data is U.S. CENSUS BUREAU, STATE & COUNTY QUICKFACTS, http://quickfacts.census.gov/qfd/download/DataSet.txt (last visited Feb. 27, 2010).

[95] In Table 6A we present a regression table with "nonwhites" (instead of combined black and Hispanic population) as the independent variable and Democratic nominee's vote share as the dependent variable. The results are even more striking. The coefficient on the nonwhite percen-

TABLE 6. RELATIONSHIP BETWEEN COUNTY RACIAL MAKEUP AND PRESIDENTIAL VOTE, 2008[96]

	Covered counties	Noncovered counties
Combined Black and Hispanic Percentage of County	0.635*** (0.022)	0.415*** (0.013)
Intercept	0.244*** (0.009)	0.461*** (0.004)
N	860	2,254
R-squared	0.496	0.307

***p<0.001. Numbers in parentheses are standard errors.

TABLE 6A. RELATIONSHIP BETWEEN COUNTY'S NONWHITE POPULATION SHARE AND PRESIDENTIAL VOTE, 2004 AND 2008

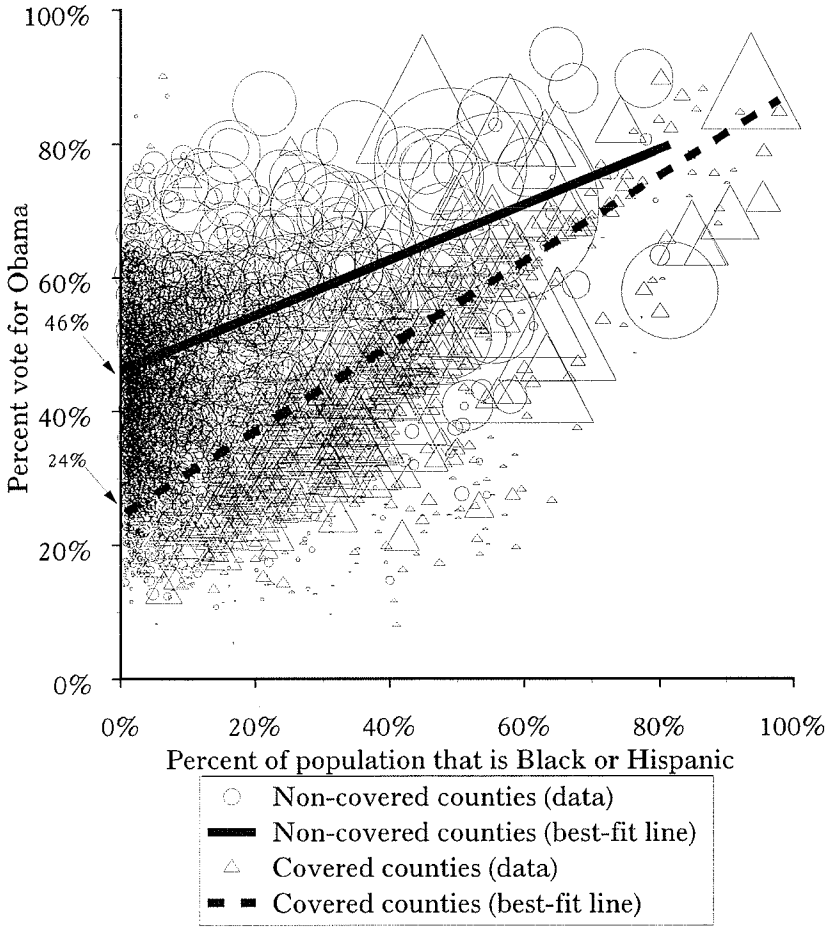
	2004		2008	
	Covered	Noncovered	Covered	Noncovered
Nonwhite percentage of county population	0.586** (0.021)	0.351** (0.011)	0.699** (0.021)	0.397** (0.011)
Intercept	0.191** (0.009)	0.404** (0.004)	0.189** (0.010)	0.441** (0.004)
N	860	2,254	860	2,254
R-squared	0.49	0.30	0.57	0.37

* p<0.05, ** p<0.01.

tage of a county grew for both the covered and noncovered counties between 2004 and 2008, although much more so for the covered counties. The increase in the coefficients and the increase in the R-squared between 2004 and 2008 reveal, again, that race played a greater role in 2008. The intercepts tell a story similar to that of the other regressions — a 4 percentage point increase in the noncovered counties and no change in the covered counties. However, the intercepts are much lower in the covered counties than they were for the regressions limited to blacks and Hispanics — 0.191 in 2004 and 0.189 in 2008. This translates into an estimate of approximately 19% support among whites in the covered counties for the Democratic nominees in 2004 and 2008.

[96] For an explanation of the data sources used in Tables 6 and 6A, see *supra* note 94.

FIGURE C. RELATIONSHIP BETWEEN COUNTY RACIAL MAKEUP AND PRESIDENTIAL VOTE, 2008



For ease of comparison of the 2004 and 2008 elections, we replot the regression lines from Figures B and C in Figure D. The intercept for the line for the covered jurisdictions remains the same between the elections, even while the line in 2008 indicates a rising vote share for Obama as the minority percentage in the county increased. For the noncovered jurisdictions, the shift upward occurs throughout the length of the line. This indicates that the increase in Obama's vote share occurred regardless of the racial makeup of the noncovered counties. To address the possibility that partisan preferences totally explain the differences between the covered and noncovered jurisdictions, we can control for a county's previous support for the Democratic nominee. Tables 7 and 8 control for the Kerry vote in explaining the Obama vote. Table 7 presents a multivariate regression, which includes only the percentage of the county voting for Kerry in 2004 and the percentage of the county that is white, in order to explain the county-based election results for 2008. Of course, most counties voted the way they did in 2004, and the results in 2004 are the best predictor of how a county would vote in 2008. Even controlling for the Kerry vote, however, does not erase the significance of the county's racial composition. A county's racial makeup remains significant for both the covered and the noncovered regressions, but the coefficient on the racial composition variable for the covered counties is about twice as large as it is for the noncovered counties (-0.152 versus -0.080).

Table 8 depicts the same phenomenon somewhat differently. The dependent variable there is the difference between Obama's vote share and Kerry's vote share, and the independent variable is the white percentage of the county. Again, the coefficient on the racial composition variable is about twice as large for the covered counties as it is for the noncovered counties (-0.111 versus -0.047). The R-squared for the regression for the covered counties is also higher, suggesting that, with respect to changes since 2004, racial composition explains more of the differences over time among the covered counties than among the noncovered counties. At the risk of belaboring the point, it is worth noting that these differences are, in no small measure, due to increased turnout and cohesion among minority voters, but the differences between whites in the covered and noncovered counties also contribute to the greater role played by race in the 2008 election.

Until this point, we have treated covered states and noncovered states as undifferentiated groups, without examining the diversity within these two classes of states. The state-level exit polls allow us to assess how well the current coverage formula captures states with the largest racial differences in vote preferences, the lowest levels of whites voting for Obama, or the greatest changes in white vote share for the Democratic nominee between 2004 and 2008. Table 9 provides all of these statistics.

FIGURE D. COMPARISON OF THE RELATIONSHIP BETWEEN COUNTY RACIAL COMPOSITION AND DEMOCRATIC VOTE SHARE, 2004 AND 2008

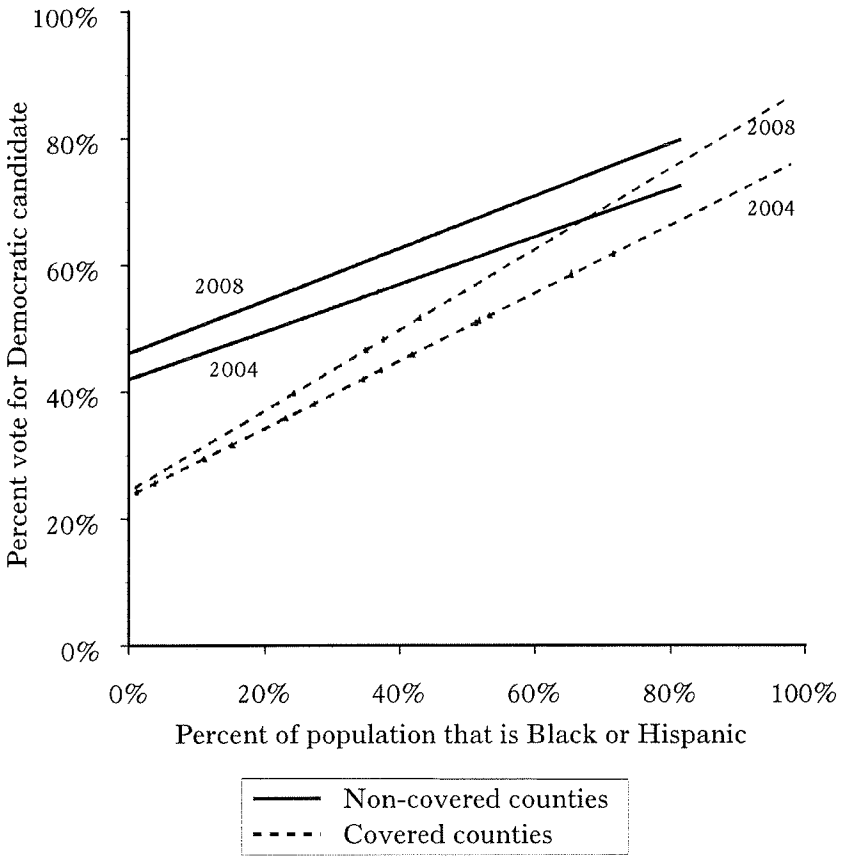


TABLE 7. RELATIONSHIP OF 2004 VOTE AND RACIAL MAKEUP TO 2008 VOTE AT COUNTY LEVEL[97]

	Covered	Noncovered
Percentage vote for Kerry in 2004	0.930 (0.014)**	0.909 (0.007)**
Percentage of population that is white	-0.152 (0.012)**	-0.080 (0.005)**
Constant	0.165 (0.012)**	0.155 (0.006)**
N	860	2254
R-squared	0.930	0.92

* p<0.05, ** p<0.01. Standard errors in parentheses.

TABLE 8. PREDICTED CHANGE IN SUPPORT FOR DEMOCRATIC NOMINEE FROM 2004 TO 2008 BY WHITE PERCENTAGE OF COUNTY

	Covered	Noncovered
Percentage of county population that is white	-0.111*** (0.009)	-0.047*** (0.004)
Intercept	0.111*** (0.005)	0.086*** (0.003)
N	860	2,254
R-squared	0.159	0.06

***p<0.001. Standard errors in parentheses.

[97] For an explanation of the data sources used in Table 7, see *supra* note 94.

TABLE 9. RACE AND VOTE CHOICE BY STATE, 2008[98]

State	% of Whites Voting for Obama	Change in White Vote Share from 2004	% of Total Vote Received by Obama	Change from 2004
Alabama	10	-9**	39	2
Mississippi	11	-3	43	3
Louisiana	14	-10**	40	-2
Georgia	23	0	47	6
South Carolina	26	4*	45	4
Texas	26	1	44	6
Oklahoma	29	0	34	0
Arkansas	31	-6**	39	-6
Utah	32	7**	34	8
Wyoming	33	4	33	4
Alaska	34	0	38	2
Idaho	34	4	36	6
Tennessee	34	0	42	-1
North Carolina	35	8**	50	6
Kentucky	36	1	41	1
Nebraska	40	7**	42	9
Virginia	39	8**	53	8
Arizona	40	-1	45	1
Kansas	40	6*	42	5
South Dakota	42	4*	45	7
West Virginia	44	2	43	0
Florida	43	1	51	4
Missouri	42	0	49	3
New Mexico	43	0	57	8

[98] States fully covered by the VRA appear in bold, partially covered states are underlined, and shaded rows denote states won by Obama. Exit poll data from 2004 and 2008 are from the ICPSR, see *supra* note 78. The cell entries for columns two and three are limited to votes cast by whites for the Republican and Democratic nominees, and the data for these entries are from 2008 state-level exit polls. See CNN, *supra* note 87. For the election results data in columns three and four, see DAVE LEIP, DAVE LEIP'S ATLAS OF U.S. PRESIDENTIAL ELECTIONS, http://uselectionatlas.org/RESULTS/index.html (last visited Feb. 27, 2010).

North Dakota	43	7**	45	9
United States	*44*	*4***	*53*	*5*
Indiana	45	11**	50	11
Montana	46	6**	47	8
Nevada	46	2	55	7
Ohio	47	3	51	2
Maryland	49	4	62	6
Pennsylvania	48	3	54	3
New Jersey	49	4	57	4
Colorado	51	8**	54	7
Connecticut	53	1	61	7
Illinois	52	3	62	7
Iowa	52	3*	54	5
Michigan	52	7**	57	6
California	53	5**	61	7
New York	53	3	63	5
Delaware	54	9**	62	9
Minnesota	54	3*	54	3
New Hampshire	55	5**	54	4
Wisconsin	55	7**	56	6
Washington	57	4*	57	4
Oregon	59	9**	57	6
Maine	59	5**	58	4
Rhode Island	60	1	63	4
Massachusetts	60	1	62	0
Vermont	69	9**	67	8
Hawaii	72	14**	72	18
D.C.	88	8	92	3

*p<0.05, **p<0.01.

All of the covered states are below the national average when it comes to the share of the white vote that Obama received. The six states with the lowest share of white voters voting for Obama are all covered states. They range from Alabama, where only 10% of whites voted for Obama, to Texas, where 26% voted for Obama. The three covered states not appearing at the lowest end of the white crossover voting spectrum are Alaska, Virginia, and Arizona. It should also be noted that the five states with the lowest levels of white crossover voting and the largest gap between whites and African Americans in terms of Obama's vote share are also the states with some of the largest African American population shares. These five states are among the top six states in terms of the share of the population that is African American. According to the 2006 Census population estimates, Mississippi (37.1%), Louisiana (31.9%), Georgia (29.6%), Maryland (28.8%), South Carolina (28.6%), and Alabama (26.3%) have the highest African American population shares of any state.[99] All but Maryland are covered by section 5 of the Voting Rights Act.

The changes between 2004 and 2008 are also revealing. In three of the covered states, the white vote for the Democratic presidential nominee decreased from 2004 to 2008. In Alabama and Louisiana the decrease was substantial and statistically significant. In Mississippi, white support for the Democrat fell 3 percentage points, but that drop was not statistically significant. In two other covered states, Obama did better than Kerry among whites. The share of white voters in South Carolina and Virginia willing to vote for the Democrat increased by 4 and 8 percentage points, respectively, between 2004 and 2008.

In a large number of the noncovered states, Obama made significant gains among white voters. These states include some, such as North Carolina, which are partially covered, where Obama received 8 percentage points more of the white vote than Kerry did. In only one noncovered state, Arkansas, did Obama experience a significant drop (6 percentage points) in the share of the white vote he received.

C. Accounting for Party and Ideology

As noted above in our discussion of the section 2 jurisprudence, partisanship and ideology are often used to explain the differences in voting patterns among racial groups. Similarly, partisanship and ideology serve as frequent explanations for why whites in the states covered by section 5 differ from whites in the noncovered states in their candidate preferences. To generalize, of the groups analyzed here, blacks have been the most Democratic-leaning group, followed by

[99] *See* U.S. CENSUS BUREAU, POPULATION ESTIMATES (2006), *available at* http://www.census.gov/popest/states/asrh/SC-EST2006-03.html.

Hispanics, followed by whites in the noncovered states, and then followed by whites in the covered states, who are the most likely to be Republican. The exit poll data suggest that partisanship reduces but does not explain away the differences among whites across the covered and noncovered jurisdictions. It is still possible that ideological conservatism or religiosity, in addition to party, could account for the differences between whites in the covered and noncovered states.

To test for this possibility, we turn to publicly available national sample surveys that measure many more ideological and issue variables and demographic characteristics than are gauged by the exit polls. The 2008 Cooperative Congressional Election Study (CCES) provides a very large, nationally representative sample (25,000 interviews),[100] allowing precise estimates of differences across areas and groups, and the 2004 and 2008 American National Election Studies (ANES), which have much smaller samples,[101] allow us to examine changes in voting behavior from an election involving two white candidates (2004) to an election involving a white and a black candidate (2008).

Although demographic and ideological variables account for much of the difference between whites in the covered and noncovered states, living in a covered state was a statistically significant factor for whites voting against Obama in 2008. *This was not the case in 2004.* The differences between whites in the covered and noncovered states could be attributed to such demographic and ideological variables when John Kerry ran against George W. Bush. The same cannot be said when Barack Obama ran against John McCain.

Table 10 presents a regression of the reported vote of whites featuring variables in the CCES data that might have an impact on vote choice, in addition to the coverage status of the state in which the respondent lived. Partisanship, ideology (self-placement on a liberal to conservative spectrum), and importance of religion exert strong influences in the expected directions. Democrats, Independents, liberals, and less religious respondents were more likely to vote for Obama, while Republicans, conservatives, and more religious respondents were less likely to do so. Education is also positively associated with support for Obama, while age, income, and being male are negatively associated. That is, older, richer, and less educated respondents, as well as male respondents, were less likely to vote for Obama.

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[100] COOPERATIVE CONGRESSIONAL ELECTION STUDY 2008, COMMON CONTENT (2009), *available at* http://web.mit.edu/polisci/portl/cces/.

[101] A description of the ANES sample sizes and designs is provided by ARTHUR LUPIA, JON A. KROSNICK, PAT LUEVANO, MATTHEW DEBELL & DARRELL DONAKOWSKI, USER'S GUIDE TO THE ANES 2008 TIME SERIES STUDY 8 (2009).

TABLE 10. DIFFERENCE IN SUPPORT AMONG WHITE VOTERS FOR OBAMA BETWEEN VRA COVERED AND NONCOVERED STATES, CONTROLLING FOR OTHER FACTORS, IN PRIMARY AND GENERAL ELECTIONS[102]

Probability Vote for Obama
(Probit Estimates)

Independent Variable	In Primary or Caucus		In General Election	
	Coef (SE)	Coef (SE)	Coef (SE)	Coef (SE)
VRA Covered State	-0.113 (0.039)*	-0.108 (0.043)*	-0.385 (.022)*	-0.177 (.035)*
Education		0.044 (0.011)*		0.080 (.009)*
Income		0.001 (0.004)		-0.018 (.004)*
Age in Years		-0.008 (0.001)*		-0.003 (.001)*
Gender		0.179 (0.031)*		-0.066 (.026)*
Democrat		-0.128 (0.152)		1.131 (.082)*
Republican		-0.062 (0.164)		-0.818 (.084)*
Independent		0.049 (0.153)		0.199 (0.082)*
Other Party		0.134 (0.217)		-0.421 (0.102)*
Ideology		-0.157 (0.020)*		-0.719 (0.077)*
Ideology Missing		-0.396 (0.106)*		-2.187 (0.077)*
Union Member		-0.105 (0.032)*		-0.010 (0.029)
Church Attendance		-0.028 (0.015)		0.003 (0.012)
Born Again		-0.171 (0.044)*		-0.187 (0.032)*
Importance of Religion		-0.072 (.019)*		-0.144 (0.017)*
Constant	-0.021 (0.015)	0.109 (0.218)	-0.077 (0.009)	1.486 (0.126)
N	8,598	7,680	22,926	21,239
Loglikelihood	-5951.733	-5109.50	-15593.03	-6365.51

*p<0.05, **p<0.01.

[102] Compiled by authors using the COOPERATIVE CONGRESSIONAL ELECTION STUDY 2008, *supra* note 100.

Even with all of these controls added, however, coverage status remains statistically significant. The coefficient drops substantially in size — from -0.385 to -0.177 — once the controls are added, but being from a covered state remains a statistically significant factor in predicting opposition to Obama. These coefficients are in a nonlinear (probit) scale. The coefficients can be used to calculate the effect of the VRA coverage variable by holding other variables in the analysis constant at their mean values and calculating the implied difference in the probabilities of voting for Obama between those states covered by the VRA and those that are not. That calculation reveals that Obama received about 15% higher vote share among whites in noncovered jurisdictions than he did in covered jurisdictions without controlling for other factors. Inclusion of the control factors shrinks that difference but does not eliminate it. Holding the other variables in the analysis constant, Obama received about 7% higher vote share among whites in noncovered jurisdictions than he did in covered jurisdictions, and that difference is statistically different from zero at the 0.01 level. In other words, party, ideology, gender, income, education, religion, and union membership explain some of the difference in vote preferences of whites between covered and noncovered jurisdictions, but these factors can account for only about half of the difference. A sizable 7 point difference remains between whites in covered and noncovered jurisdictions who have the same party, the same gender, the same educational attainment, the same income, the same union status, the same religious orientation and conviction, and the same political ideology.

Demographic, partisan, and ideological variables do, in fact, account for the differences between whites in the covered and noncovered states in 2004. In other words, when Kerry was the Democratic nominee, the differences between whites in the covered and noncovered states could be explained by whites in the covered states being more Republican, conservative, or religious. In 2008, the other demographic, partisan, and ideological variables did not explain the independent significance that being from a covered state had on predicting a vote against Obama.

Tables 11 and 12 present data from the National Election Studies (NES) for 2004 and 2008, respectively. The regressions are similar to the one provided in Table 10, but use the smaller and more limited NES dataset. Being from a covered state is statistically significant in 2008 but not in 2004. The coefficient (-0.078) is negative and statistically significant (at the 0.01 level) for 2008, meaning that whites in the covered states (all other things being equal) were about 7.8 percentage points less likely to vote for Obama. For 2004, the coefficient (0.003) is insignificant and positive.

Once again, these differences do not prove that Obama's race "caused" whites in the covered states to be more likely to vote against him. There could be any number of other variables that are omitted

TABLE 11. DIFFERENCE IN SUPPORT AMONG WHITE VOTERS FOR JOHN KERRY BETWEEN VRA COVERED AND NONCOVERED STATES, CONTROLLING FOR OTHER FACTORS[103]

Independent Variable	Coef (SE)
VRA Covered State	0.003 (0.049)
Education	0.044 (0.089)
Income	0.032 (0.075)
Age in Years	0.001 (0.001)
Gender	-0.008 (0.043)
Democrat	0.243 (0.114)*
Republican	-0.303 (0.115)**
Independent	-0.004 (0.114)
Ideology	0.688 (0.112)**
Church Attendance	-0.005 (0.073)
Importance of Religion	-0.049 (0.074)
Union	0.054 (0.047)
Constant	0.157 (0.174)
N	324
Adjusted R-squared	0.522

*p<0.05, **p<0.01.

[103] Analysis conducted by the authors. For data, see UNIV. OF MICH., CTR. FOR POLITICAL STUDIES, THE NATIONAL ELECTION STUDIES, 2004 AMERICAN NATIONAL ELECTION STUDY, http://www.electionstudies.org/studypages/2004prepost/2004prepost.htm.

TABLE 12. DIFFERENCE IN SUPPORT AMONG WHITE VOTERS FOR BARACK OBAMA BETWEEN VRA COVERED AND NONCOVERED STATES, CONTROLLING FOR OTHER FACTORS[104]

Independent Variable	Coef (SE)
VRA Covered State	**-0.078 (0.033)****
Education	-0.049 (0.127)
Income	-0.104 (0.066)
Age in Years	0.000 (0.001)
Gender	0.045 (0.032)
Democrat	0.398 (0.087)**
Republican	-0.152 (0.083)
Independent	0.154 (0.085)
Ideology	0.639 (0.079)**
Church Attendance	0.043 (0.058)
Importance of Religion	-0.071 (0.053)
Union	0.042 (0.045)
Constant	0.162 (0.155)
N	476
Adjusted R-Squared	0.560

*p<0.05, **p<0.01.

[104] Analysis conducted by the authors. The source of this data is UNIV. OF MICH., CTR. FOR POLITICAL STUDIES, THE NATIONAL ELECTION STUDIES, 2008 AMERICAN NATIONAL ELECTION STUDY, http://www.electionstudies.org/studypages/2008prepost/2008prepost.htm.

from this analysis, and as such, the data do not prove that reaction to Obama's race is what distinguishes whites in the covered states from those in noncovered states. Abigail Thernstrom suggests that the drop in support among whites for Obama could be explained by him sounding "weak on national defense, and far to the left on domestic policies such as health care," the fact that "he talked openly about a redistribution of wealth," "[h]is emissions-trading proposal for global warming," and "his support for 'card-check.'"[105] At the same time, Kerry was a "decorated combat veteran" whose campaign did not emphasize environmental issues, and he "had no equivalent to the Reverend Jeremiah Wright in his history — a figure surely more off-putting to southern whites than to, say, Massachusetts voters."[106]

Although we question whether this characterization accurately describes the campaigns and voters' perceptions of the candidates, we must admit that survey data do not exist to allow us to disprove all of these possibilities. What data we are able to assemble regarding these claims cast doubt on them. Adding the NES variables that measure fear of "big government" does not affect the results. Moreover, to believe these alternative hypotheses, one must assume that these differences are not picked up by self-placement as a liberal or conservative and that whites in the covered states are systematically different from whites in the noncovered states along these dimensions. In addition, to prove these arguments, it is wrong to focus simply on the relative preference for Kerry over Obama, while ignoring the relative strength of their opponents. What requires explanation is why Obama did worse among whites in some states despite the fact that he was running against a much less popular candidate in an environment that was far more favorable for the Democratic nominee than four years earlier.

This debate highlights the problem we identified earlier concerning the propriety of multivariate analysis to prove racial polarization under section 2 of the VRA. Some variable other than race almost always could explain differences in candidate preferences because no two candidates are identical along every nonracial dimension. Even if all such points of difference could be accounted for, one still could not make the argument that a voter's race "caused" her to vote a particular way. In the end, these statistics can only establish associations between the political preferences of groups of voters as defined by certain characteristics. Few voters go into the voting booth with the mindset that they will vote against a candidate specifically because he is the "candidate of choice" of the minority community.

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105 THERNSTROM, *supra* note 51, at 201.

106 *Id.*

D. Race and Vote Choice in the 2008 Primary

If one seeks to generalize from the 2008 presidential election to gauge the potential for minority candidate success in individual states, one must account for both the primary and the general election results. In the context of section 2 litigation, for instance, advocates and scholars frequently argue that minority control of the Democratic primary is often the critical barrier to surmount for the minority to elect its preferred candidate.[107] Indeed, given that Democrats maintained a monopoly on Southern politics for most of the last century and a half, the Democratic primary was the only election that tended to matter. Even in the age of active partisan competition in the South and elsewhere, the hurdle of the Democratic primary often remains the more important obstacle to overcome given that a sufficient number of Democratic-leaning whites will support their party's nominee, whatever his or her race.

Reexamining the Obama victory to account for the two-stage election on a state-by-state basis, one cannot help but be surprised by the small number of states in which Obama won both the nominating contest and the general election. As Table 13 shows, in only 15 states and the District of Columbia did Obama win both the nominating event (primary or caucus) and the general election. In most states, either Hillary Clinton beat him in the nominating contest or John McCain beat him in the general election. This is important because for all other elections, a candidate must get through both the primary and general election in the same state. If either is a barrier, then the candidate will not win.

The geography of Obama's success also bears on the relevance of the coverage formula of section 5. The only fully covered state in which he won the primary and general election was Virginia, and the only partially covered state was North Carolina. Due in large part to the size of the black population in the covered states, he was able to win the primary or caucus in every fully covered state, with the exception of Arizona. But as was typical for most previous Democratic nominees, those states' large minority populations were not enough to overcome the large gap in preferences among whites in the general election. In contrast, although he won several noncovered states in both the primary and the general elections, even those represent a relatively small slice of the American population.

The structure of Obama's support in the primaries and caucuses differed somewhat from his support in the general election, as Table 10

[107] *See* Grofman et al., *supra* note 57 at 1392–93 (highlighting the importance of the primary as a hurdle for minority-preferred candidates).

TABLE 13. COMPARISON OF PRIMARY AND GENERAL ELECTION OBAMA VICTORIES BY STATE VRA STATUS

	Primary or Caucus Wins	*General Election Wins*	*Primary and General Election Wins*
Covered	**Alabama, Alaska, Georgia, Louisiana, Mississippi, South Carolina, Texas, Virginia**	**Virginia**	**Virginia**
Partially Covered	North Carolina	California, Florida, Michigan, New Hampshire, New York, North Carolina	North Carolina
Noncovered	Colorado, Connecticut, Delaware, District of Columbia, Hawaii, Idaho, Illinois, Iowa, Kansas, Maine, Maryland, Minnesota, Missouri, Montana, Nebraska, North Dakota, Oregon, Utah, Vermont, Washington, Wisconsin, Wyoming	Colorado, Connecticut, Delaware, District of Columbia, Hawaii, Illinois, Indiana, Iowa, Maine, Maryland, Massachusetts, Minnesota, Nevada, New Jersey, New Mexico, Ohio, Oregon, Pennsylvania, Rhode Island, Vermont, Washington, Wisconsin	Colorado, Connecticut, Delaware, District of Columbia, Hawaii, Illinois, Iowa, Maine, Maryland, Minnesota, Oregon, Vermont, Washington, Wisconsin

depicts, but important parallels remain.[108] Support for Obama was higher among Democratic primary and caucus voters who were more highly educated, less religious, younger, more liberal, and non-union members; not surprisingly, he did better among males than females. The key question, though, is whether Obama's support among white Democratic primary and caucus voters differed between covered and noncovered states. The analyses indicate that there is a statistically significant difference between these types of jurisdictions, and it is unaffected by the control variables included in the model. Without any controls for party, ideology, and demographics, Obama won approximately 4 percentage points more of the reported vote among whites in primaries and caucuses in noncovered states than he did among whites in primaries and caucuses in covered states. Including party identification, ideology, and demographic indicators did not alter that effect. The difference remains approximately 4 percentage points (coefficient of -0.108) and statistically different from zero at the 0.01 level.

This difference is important for two reasons. First, analysis of the primary electorate offers one way of holding party constant. The primary electorate consists of people who have chosen to participate in the Democratic Party's nominating process. They are, in that respect, Democrats. Second, and more to the substance of the legal questions involved, these results suggest that the primary election can create a second layer of difficulty for minority candidates. Observed differences in the reported preferences of white Democratic primary voters, even after controlling for their ideology, partisanship, and demographic characteristics, are correlated with the existing coverage formula.

To some extent, voter preferences did differ based on whether the respondent participated in a caucus or primary. Obama's successful strategy in the caucus states is now well known.[109] When we expand our analysis beyond whites, as in Table 14, we can gauge the disparities in the Obama primary and caucus vote based on race, type of nomination method, and coverage status. In states that employed primaries, whites and Hispanics in the noncovered jurisdictions were somewhat more likely to support Obama, but not by much. In the primary states, whites and Hispanics did not differ much from each other in their support for Obama, but, as is well known, they gave much less support to Obama than did African Americans. The caucus states seem to differ from the primary states, as African Americans

[108] As suggested in the tables, these figures come from the 2008 CCES and represent self-reporting that biased the results in favor of Obama. In other words, more respondents in the survey said they voted for Obama in the primary and caucus than the election results suggest. Nevertheless, the bias should not affect the regional differences we observe.

[109] *See* Nick Timiraos, *Obama's Strategy for Low-Turnout Caucuses Helps Drive Delegate Edge*, WALL ST. J., May 15, 2008, at A6.

TABLE 14. PERCENT OF VOTERS BY RACE REPORTING THAT THEY VOTED FOR OBAMA IN DEMOCRATIC PARTY PRIMARIES AND CAUCUSES, VRA COVERED AND NONCOVERED STATES[110]

	Primaries		Caucuses	
Race	Covered	Noncovered	Covered	Noncovered
White	46.7	48.8	50.3	60.1
Black	83.7	83.4	91.0	83.5
Hispanic	45.4	54.1	23.3	63.6

[110] Compiled by authors using the 2008 COOPERATIVE CONGRESSIONAL ELECTION STUDY, *supra* note 100.

there reported voting in even higher percentages for Obama. Greater disparities also seem to exist based on coverage status for whites and Hispanics in the caucus states, as those in the noncovered caucus states report voting at higher levels for Obama. This might be a spurious relationship based on the particular idiosyncrasies of the smaller number of states employing caucuses, the much lower and selective turnout in such states, and the small number of respondents from those states in the survey sample. The gaps between African Americans and all others still remain.

CONCLUSION: CHANGE IN VOTING BEHAVIOR WE CAN BELIEVE IN?

Nothing in the foregoing should take away from the monumental significance of the 2008 election. The election of an African American President represents a historic event by any measure. Even conceding Barack Obama's extraordinary campaign and candidacy, his success contradicts long-held assumptions about what was possible in American democracy.

The question for us is whether the results from that election suggest a transformation of relevance to voting rights law. Our general answer is no. The 2008 election did not indicate a disruption of well-known patterns of race, region, and vote choice. If anything, Barack Obama's higher vote share among minorities and his uneven performance among whites suggest those patterns are quite entrenched.

If racially differential voting patterns were to be the criteria for coverage under a new section 5 of the VRA, however, the list of covered states would need to be changed. Virginia would be dropped and Arkansas added, for example. It is one thing to point out, as this Article does, that the covered states, as a group, exhibit larger racial gaps in candidate preferences and fewer whites willing to vote for minority-preferred candidates. It would be quite another thing to say that the coverage formula completely and exclusively captures the most polarized polities. It clearly does not, nor was it ever expected to do so.

Obama's success highlights when racially differential voting patterns make a difference and when they do not. In some states, a sufficient number of whites were willing to vote for him so he did not need to rely on minority voters to cast the decisive or pivotal votes. In other states, his mobilization of minorities (and perhaps the demobilization of whites) overcame the effect of any polarization that existed in the electorate. In still others, particularly and ironically those with large African American populations, mobilization of minorities could not make up for the low share of the white vote he garnered.

For various reasons, the Voting Rights Act should be transformed. Indeed, specifically to provide greater protection for minority voting rights, we would support a fundamental rethinking of basic compo-

nents of the Act and how it structures American politics. Results from the 2008 election should not be the cause for that undertaking, however. The election of an African American as President is significant in its own right, not because it casts doubt on the VRA's continued utility or constitutionality.

Exhibit D

Exhibit D

11 Election L.J. 490

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Featured Topic: Major Developments in Redistricting

THE EFFECTS OF REDISTRICTING ON INCUMBENTS

Stephen Ansolabehere, James M. Snyder, Jr. [a1]

ABSTRACT

We analyze the effects of redistricting on the electoral fortunes of incumbent legislators, using voting data on U.S. congressional districts, state legislative districts, and statewide races. We find little evidence that redistricting helps incumbents in U.S. legislative elections. If anything, redrawing district lines reduces the average vote margin of those in districted offices compared with offices that are not districted, reduces electoral security, and increases turnover in the legislature.

1. INTRODUCTION

OVER THE PAST 50 YEARS U.S. elections have witnessed the rising importance of incumbency. While party remains the central and most important predictor of voting behavior, American voters now also base their decisions on the individuals running for office and what those individuals have done to serve local interests. Sitting legislators, when they run for reelection, evidently enjoy substantial electoral advantages, which are manifest both in voters' behavior and in aggregate rates of electoral competition and legislative turnover. For their part, voters favor incumbents, and as a result incumbents today receive vote margins approximately 10 percentage points greater than candidates running in open seats. Aggregate turnover in U.S. legislative elections has similarly fallen. Many fewer incumbents retire today than in previous generations; reelection rates of those who do run for reelection exceed 90 percent. As a result, turnover in U.S. legislatures is extremely low and the tenure of the typical legislator grows longer.

The rising advantages of incumbency, whatever their causes, have altered the patterns of representation and electoral responsiveness in the U.S. The incumbency effect has muted the effects of short-run factors, such as economic fluctuations, on the composition of U.S. legislatures (Mayhew, 1974). It has insulated the majority party in Congress and lengthened the time that the majority party can expect to remain in power (Ansolabehere and Gerber, 1997). It is thought to reflect a lack of collective responsibility inside the legislature, especially where matters of budgeting are concerned (Fiorina, 1980).

Why American legislators win reelection at such high rates remains an unsolved puzzle. Here we examine one of the most enduring and controversial conjectures--namely, that redistricting accounts for the rise of incumbent-centered politics. Incumbency arose at the same time as the redistricting revolution in the U.S., set off by the landmark U.S. Supreme Court case *Baker v. Carr*. [1] That case, decided in March 1962, led to the redrawing of nearly every U.S. House and state legislative district in America within a four year span. [2] Research in political science identifies this as precisely the time period with the steepest rise in incumbents' vote margins (Jacobson, 1987). The coincidence of rising ***491** incumbency advantages and widespread court-mandated redistricting has led many observers to conclude that the two are inextricably linked.

Three specific arguments claim a causal relation between redistricting and incumbency. First, incumbents use redistricting to create safe (partisan) seats. Perhaps the clearest statement of this argument comes from Samuel Issacharoff (2002), who likens

redistricting to foxes guarding the hen house. The two major parties, he argues, engage in duopolistic behavior to guarantee their continued dominance of American elections by creating large numbers of safe Democratic and safe Republican seats. [3] Second, incumbents use redistricting to protect their own seats. Redistricting turns the normal representation process on its head. It is at this moment that politicians choose their constituencies, rather than the constituencies choosing the politicians. That opportunity might allow incumbents to create districts that are particularly favorable to them on personal grounds. Third, redistricting creates incumbency advantages by creating barriers to entry for challengers. Political control of redistricting allegedly gives incumbents another instrument with which to deter entry. As a result, new district boundaries are thought to increase legislators' vote shares. Note that this argument is distinct from traditional partisan gerrymandering, in which the majority party creates more seats for itself by "packing" voters of the opposition party. Under partisan gerrymandering seats held by the minority party's incumbents will tend to get safer, but districts held by the majority party's incumbents may become more competitive. [4]

Alan Ehrenhalt, executive editor of *Governing* magazine, offered the following succinct assessment:

> Decades of litigation and judicial activism have created a system in which bizarrely shaped districts exist to serve undisguised racial purposes. Partisan gamesmanship has brought us legislatures stacked with safe seats that preclude competition at election time. All of this has taken place under the auspices of a Supreme Court doctrine that virtually any political outrage is permissible as long as the census populations of the districts are mathematically the same--even though no such mathematical precision even exists. [5]

These arguments represent an important line of thinking about the political consequences of allowing legislators to draw district boundaries. In one sense, they are right. An obvious conflict of interest arises when a politician chooses his or her constituency, and legislators devote considerable effort to influence the composition of their districts. The presence of such a conflict of interest does not translate necessarily into a political advantage, however.

What actually happens to incumbents around redistricting is quite a different picture. Redistricting is a disruptive factor for incumbents and parties. Looking at electoral outcomes across redistricting periods we conclude that periodic redistricting forced by the courts since the 1960s has, on the whole, weakened incumbency and partisan electoral advantages.

We examine five key indicators of the consequences of political control over electoral institutions. The first is the partisanship of districts. Insulation of incumbents and of the incumbent party predicts that most districts are either overwhelmingly Democratic or overwhelmingly Republican, with few mixed or moderate constituencies. Using conventional measures we show that the opposite is true--most districts are moderate and there is no or little bimodality in the distribution of districts' partisanship.

The second indicator is vote differentials between "new" and "old" parts of incumbents' districts. Changes in district boundaries hurt incumbents since they lose voters whom they had served and gain voters who don't know them. There is, in fact, a large difference between the votes that incumbents win among familiar and new parts of their districts. (See Ansolabehere, Snyder, and Stewart, 2001.)

The third indicator is the incumbency effect on the vote, estimated either using regression analysis or sophomore surge. The incumbency effect is defined as the expected difference between the vote share won by a party when an incumbent is running and when an incumbent is not running. This quantity can be estimated for any type of office, such as all U.S. House elections or all U.S. Senate elections. (See Erikson, 1971; Gelman and King, 1990) We compare the incumbency effects for offices that are districted and offices that are not districted, and ***492** find that the incumbency effect is in fact much smaller in districted elections (state legislatures and the U.S. House) than in non-districted elections (U.S. Senate, governor, and other statewide offices).

The fourth indicator is changes in district partisanship. We find small changes in partisanship, and in the case of Democrats, changes that hurt incumbents on average rather than help them. Persily (2002) makes a similar observation in response to Issacharoff. [6]

Finally, we examine turnover. If redistricting amounts to incumbency protection then there should be little or no turnover in the elections following the creation of new constituencies. In fact, redistricting years regularly show the highest turnover.

We present a series of statistical analyses and show that in one analysis after another the data do not break in the direction one would expect if incumbents were able to carefully control the process and carve up the electoral terrain to their benefit. There are, of course, exceptions to this overall picture, but the typical state legislator or member of Congress dreads the redistricting process. It disrupts relations that the legislator has built up with a constituency over the course of a decade. Those disruptions, though, are a necessary part of democratic politics; they are required in order to allow Congress and state legislatures to evolve with the ever changing American electorate.

2. PARTISANSHIP OF DISTRICTS

There are really two distinct arguments. The first alleges that incumbents of both parties have raised their electoral security by crafting safe partisan districts, with overwhelming numbers of Democrats or of Republicans. The second claims that incumbents are able to increase their personal vote through the redistricting process. In this section we turn to the first of these two conjectures. We examine the second possibility in the sections that follow.

Isaacharoff, writing in the *Harvard Law Review*, offers the most cogent expression of this idea, which he calls duopolistic gerrymandering. The two parties he argues have formed something of a cartel, a duopoly through which they decide to divide the political turf to suit each party's interest and fend off all potential entrants into their market. Most states are closely divided between the parties. Arbitrarily drawn districts would create a large number of districts that are split evenly between Democratic and Republican adherents, and with a good dose of Independent voters thrown in as well. For politicians, this would be a terrible state of affairs as competitive districts are difficult to win and hard to hold onto. Democratic and Republican legislators, realizing this problem, may join forces and divide their state so that most seats are safe for one party or another, leaving only a handful of evenly split, competitive districts. Focusing on the facts in *Gaffney v. Cummings*, Issacharoff argues that the "conceptual weakness in how the Court has treated the potential for mischievous manipulation of redistricting is evident in.a political compromise between the Democrats and Republicans of Connecticut to partition the state so as to lock in the political status quo ante." [7]

This is a nice argument. Sitting legislators can improve their own political positions and reduce the risk facing their party. Duopolistic gerrymandering buys the support of the large majority of sitting legislators since the large majority of them are guaranteed safe (partisan) seats. Such a plan minimizes the electoral uncertainty and electoral costs that the parties face, since competition is limited to a few places. It does not cause incumbency advantages, but it would have the side effect of protecting those incumbents who happen to be in safe Democratic and safe Republican seats. It would, if real, deepen the partisan division among the constituencies that the legislators represent and further polarize the parties inside the legislature. Of course it assumes that the members of the minority party would prefer to remain a permanent legislative minority, safe in their own seats but with little hope of gaining control of the house.

This is not just the stuff of academic debates. It is widely believed to be the new reality. Jeffrey Toobin, writing in the *New Yorker* magazine, has called this "The Great Election Grab." Republican congressman James Leach of Iowa said of Congress in 2003 that "a little less than 400 seats are totally safe." Even those who had once defended the current process now think it broken. The sharp increases in incumbent departures following redistricting cast a ***493** shadow of doubt over this conjecture. Patterns of defeat increase those doubts still further. It is indeed true that in the typical election only one in ten seats are highly competitive, the set of close elections varies from year to year-- hardly what one would expect from a carefully crafted set

of boundaries. Indeed, three years after Congressman Leach offered his assessment, the voters bounced Leach and 30 of his Republican colleagues, giving Democrats control of Congress for the first time in 12 years.

The districts themselves provide direct evidence that the politicians have not been able to capture the redistricting process in the ways that Issacharoff, Toobin, and others conjecture. It is not that they haven't tried, they just haven't succeeded wholesale. Successive rounds of duopolistic gerrymandering over the past 40 years should have created large numbers of highly safe Democratic seats and highly safe Republican seats. The result would be a small number of marginal districts in which the ratio of Democrats to Republicans is about even, and a very large number of districts where the fraction of Democrats far exceeds the fraction of Republican identifiers or vice versa. If one were to rank order the legislative districts in the country from most Democratic to most Republican and count the frequency of districts that were safely Democratic, marginal, or safely Republican, a two-humped or bimodal shape would occur. Such a bimodal distribution of the underlying partisanship of district partisanship would effectively insulate incumbents against national swings in the two-party vote. In other words, if the distribution of district partisanship were bimodal, then a national swing in the vote of, say 5 or 10 percentage points from the Democrats to the Republicans or vice versa, would produce relatively few incumbent defeats.

By contrast, the districts may be drawn with little regard for or opportunity to create safe seats or protect incumbents. The bargaining over redistricting across legislative chambers and with the executive maybe so intense and subject to so many competing demands that it leaves little leeway for party. If the lines were drawn without regard to party, the typical district would reflect the division of the vote statewide, and the partisan division in most of the districts would deviate only somewhat from the division of the statewide vote. There would be a few highly Republican and highly Democratic districts. This principle has been conjectured, at least among statisticians, for over a century. The eminent nineteenth century statistician F.Y. Edgeworth studied English parliamentary elections with just such a hypothetical district system in mind and predicted that if redistricting did not matter we should expect a bell-shaped, or normal, curve. Under this possibility, a swing of 5 or 10 points nationwide would turn out of office relatively large numbers of incumbents. Conventionally, political scientists have used margins of 60-40 or 55-45 to define which districts are marginal.

Which view is right? Fig. 1(a) and Fig. 1(b) present the distribution of the underlying (or normal) party division of the vote in the American state legislatures during the 1980s. This decade provides an ideal testing ground for the claims of duopolistic gerrymandering. The 1980s represent the highwater mark for the incumbency advantages in U.S. House elections, and, by the beginning of this decade, the questions about equality of legislative district populations had largely had been settled and the initial shock waves of the reapportionment revolution had damped down. Professor Gary King and his colleagues at Harvard University developed a unique database of the precinct-level election returns for the years 1984 to 1990 for almost all state legislatures, called the ROAD database. We used these data to construct the partisan division in each state legislative district in the United States. For each district in each state in the 1980s we calculated the average Democratic share of the two-party vote in all offices. That resulted in 50 separate graphs. We laid each of those graphs over each other; that is, we centered all of the states at 50 percent and made a single graph of all state legislative districts. This allows us to examine the distribution of the party division of the vote in all of the states at once. That distribution is shown in Fig. 1(a) for lower houses, and in Fig. 1(b) for upper chambers. [8]

TABULAR OR GRAPHIC MATERIAL SET FORTH AT THIS POINT IS NOT DISPLAYABLE

FIG. 1(a). Distribution of underlying party division of the vote in American state legislatures during the 1980s (lower houses). 1(b). Distribution of underlying party division of the vote in American state legislatures during the 1980s (upper chambers).

***494** The distribution of the vote across legislative districts is clearly unimodal. Most of the districts are near the center of the distribution, and the frequency of districts tails off as one moves away from the political center. To put matters bluntly, there is absolutely no trace of the bipartisan cartel. Rather the distribution of the normal party vote in the legislative districts looks to be consistent with the notion that the parties can exert relatively little influence over the contours of legislative districts. They

have some influence on the margin, but, consistent with our earlier findings on partisan bias, state legislative districts do not seem to reflect a strongly partisan tilt.

Finally, we turned our sights on the U.S. House, using the presidential vote. This is shown in Fig. 2. Nearly the same picture arises in the 1980s, 1990s, and 2000s. The distribution of the normal vote in the House follows a normal curve very closely. There is a slight asymmetry, a small cluster of extremely Democratic seats. These seats are created to ensure minority representation, and we will discuss them below. Otherwise the distribution of the partisan division of the vote in U.S. House elections resembles that in the state legislatures. The large majority of districts are fairly evenly split between the two parties. Most districts have partisan divisions that lie in the range between 40% Democratic versus 60% Republican to 40% Republican versus 60% Democratic. [9] At least in their underlying partisan composition, most U.S. House and state legislative seats are competitive.

TABULAR OR GRAPHIC MATERIAL SET FORTH AT THIS POINT IS NOT DISPLAYABLE
FIG. 2. Distribution of normal vote in the House of Representatives, using the presidential vote.

That is precisely the picture one would expect from a system in which legislators could not create safe seats for themselves. The division of the pie that duopolistic gerrymandering would have produced did not emerge, except in isolated cases such as New York's senate. The district maps do not polarize the electorate. Rather they represent the typical voter in the state quite well. Most of the districts cluster near the average division of partisan preferences, which happens to be about 50-50.

Of course, this is not to say that parties are ineffective at helping themselves when they control the redistricting process. As documented in Ansolabehere and Snyder (2008) and elsewhere, measures of partisan bias are substantially higher when there is unified party control of state government and the legislature controls redistricting. However, over the past five decades these biases have actually shrunk. Thus, the fact that state legislatures are now forced to draw new districts every 10 years has not led to a marked increase in either the skew or bimodality in the distribution of district partisanship.

3. OLD VS. NEW AREAS

The second form of the argument that redistricting causes incumbents' electoral advantages alleges that the districts themselves are carved up in a way that creates a personal vote for the individual representative. That might come from the creation of a district that contains voters loyal to the incumbent ***495** from past service and name recognition or from the creation of a district with higher numbers of voters inclined to favor the incumbent by virtue of party or for sociological affinities (such as ethnicity or religion).

We look at each of these factors separately. The approach in this section is to examine how redistricting alters the constituencies represented by incumbents and, in turn, changes their vote shares.

3.1. Changing partisanship of districts

One line of thinking holds that each redistricting makes marginal changes in the partisanship of districts. Through successive rounds of redistricting incumbents gain more favorable partisan compositions of their districts. One important branch in this vein reflects the political control of the institutions that conduct or oversee the redistricting. In some states the legislatures draw lines; in others lines are drawn by independent commissions; in still others the duty falls to the courts. Does redistricting improve the partisan composition of the typical incumbent's districts? Does political control of redistricting make for more partisan districts?

For the three most recent redistricting episodes, we measure directly the degree to which redistricting helps incumbents by shifting the partisan composition of their districts in their favor. We cannot directly measure party identification at the congressional district level, but we can use the presidential vote. [10]

Importantly, for the three most recent episodes, voting data are available for the *same* presidential election in both the old and new districts after redistricting. Specifically, the results of the 1980 presidential election are available for the districts that applied in 1980 and also the districts that applied in 1982; the results of the 1988 election are available for the both the 1990 and 1992 districts; and the results of the 2000 election are available for both the 2000 and 2002 districts.

***496** Given this data, the analysis is straightforward and proceeds as follows. Fix a particular redistricting episode. Let $V^B{}_i$ be the Democratic percentage of the two-party vote in the relevant presidential election in incumbent *i*'s district before redistricting, let $V^A{}_i$ be the Democratic percentage of the two-party vote in the relevant presidential election in incumbent *i*'s district after redistricting, and let $\#_i = V^A{}_i - V^B{}_i$ Let S_D (S_R) be the set of districts held by Democratic (Republican) incumbents running for reelection in the year after redistricting, and let N_D (N_R) be the number of districts in S_D (S_R). Finally, let $_{\Delta\#D} = (1/N_D)\ \#_{i\#S}D\ \#_i$ and let $_{\Delta\#R} = (1/N_R)\ \#_{i\#S}R\ \#_i$ (note the reversal of sign for Republican incumbents). These last quantities are the main quantities of interest, and provide measures of the average extent of pro-incumbent or antiincumbent partisan shifts in districts. In both cases, positive values imply changes in district partisanship that favor incumbents.

The results are shown in Table 1. We find that, on average, redistricting slightly helps Democratic incumbents in terms of district partisanship, and slightly helps Republican incumbents. Specifically, districts held by Democratic incumbents become about 0.5 percentage points *less* Democratic after redistricting, and districts held by Republican incumbents become about 1 to 1.5 percentage points *more* Republican. Neither of these changes is overwhelming. That is, those drawing district lines indeed try to help incumbents, but the effects are on the order of one percentage point.

TABLE 1. CHANGE IN DISTRICT PARTISANSHIP DUE TO REDISTRICTING

	1980-1982	*1990-1992*	*2000-2002*	*Pooled*
States with 2 + districts:				
Democratic Incumbents	-0.67 [a1] (216)	-1.00 [a1] (223)	0.14 (193)	-0.54 [a1] (632)
Republican Incumbents	0.91 [a1] (164)	1.48 [a1] (139)	1.13 [a1] (190)	1.16 [a1] (493)
States with 8 + districts:				
Democratic Incumbents	-0.90 [a1] (172)	-1.04 [a1] (174)	0.35 (157)	-0.56 [a1] (503)
Republican Incumbents	1.18 [a1] (121)	2.05 [a1] (104)	1.37 [a1] (145)	1.50 [a1] (370)

Cell entries are $_{\Delta\#D}$ for Democratic incumbents and $_{\Delta\#R}$ for Republican incumbents. Entries in parentheses are ND and NR, respectively. See text for details.

Footnotes

a1 = cell value is statistically different from zero at the .05 level.

Finally, there is the question of agency. Who does the redistricting might determine the effects on the electoral process. Table 2 considers whether itmatters if redistricting is done by state legislatures, commissions, or courts. [11] Carson and Crepsin (n.d.) argue that plans drawn by commissions and courts generally lead to more electoral competition than those drawn by state legislatures. Overall, we find little differences with respect to changes in district partisanship. In none of the four rows are the F-statistics for tests of the equality of the three cell means statistically significant from zero at the .05 level. [12] The one noticeable difference is for Democratic incumbents under courtordered plans (row 1 and row 3). In this case district partisanship shifts against the incumbent by more than one percentage point, on average, while under legislative or commission plans the changes are minimal. On the other hand, court-ordered plans are not much different than other types for Republican incumbents. Thus, the findings by Carson and Crespin are likely due to factors other than district partisanship, such as race or ethnicity.

TABLE 2. CHANGE IN DISTRICT PARTISANSHIP DUE TO REDISTRICTING BY TYPE OF REDISTRICTING BODY

	Legislature	*Commission*	*Court*
States with 2 + districts:			
Democratic Incumbents	-0.34 (394)	-0.20 (96)	-1.31 (142)
Republican Incumbents	1.19 (314)	1.19 (74)	1.01 (105)
States with 8 + districts:			
Democratic Incumbents	-0.35 (310)	-0.25 (75)	-1.29 (118)
Republican Incumbents	1.66 (239)	1.60 (53)	0.94 (78)

All three redistricting episodes pooled. Cell entries are $_{\Delta\#D}$ for Democratic incumbents and $_{\Delta\#R}$ for Republican incumbents. Entries in parentheses are N_D and N_R, respectively. See text for details.

Table 3 takes a closer look at the plans drawn by state legislatures, divided according to the party in control of the state government at the time of redistricting. A state is under Democratic control if Democrats have a majority in both houses of the state legislature the governor is a Democrat, or if Democrats have veto-proof majorities in both houses of the state legislature. Republican control is defined analogously, and the remaining cases are classified as Divided. There is some evidence of "packing" in Table 3. Democratic incumbents are *helped* by redistricting when Republicans control the state government, but not in other cases. Also, while Republican incumbents are helped in all cases, they are especially helped when Democrats control the state government. In all four rows of the table the F-statistics for tests of the equality of the three cell means are statistically significant from zero at the .05 level.

TABLE 3. CHANGE IN DISTRICT PARTISANSHIP DUE TO REDISTRICTING BY PARTY CONTROL (LEGISLATIVE PLANS ONLY)

	Democratic	*Divided*	*Republican*
States with 2 + districts:			
Democratic Incumbents	-0.55 (185)	-0.49 (104)	1.83 (48)
Republican Incumbents	2.26 (109)	0.82 (82)	0.74 (64)
States with 8 + districts:			
Democratic Incumbents	-0.55 (145)	-0.67 (75)	2.61 (35)
Republican Incumbents	3.01 (87)	1.50 (50)	0.91 (52)

All three redistricting episodes pooled. Cell entries are$_{\#\#D}$ for Democratic incumbents and $_{\Delta\#R}$ for Republican incumbents. Entries in parentheses are N_D and N_R, respectively. See text for details.

***497** Note that the analyses above focus on incumbents who run for reelection. Thus, they may understate the degree to which redistricting hurts incumbents. This would be the case, for example, if redistricting also induces retirements, especially among incumbents whose districts shift more sharply against them. The following quote is illustrative: "Wolpe decided against either of two unpalatable alternatives: a primary fight against Democrat Bob Carr in the 8th, or a run in the new 7th, which was at least 5% more Republican than his old seat and in which about half the voters had no acquaintance with the hard-driving constituency service which has offset his liberal voting record." [13] In future work we will analyze data on the districts in which retiring incumbents *would* have run had they run for reelection, which will allow us to check this.

To the extent that we find that redistricting increases the partisan vote share of incumbents, the effects are small. On average, a typical Democratic incumbent's average district partisanship increases by .5 percentage points, and a typical Republican incumbent's average district partisanship increases by one percentage point. When one party controls the entire process the effects are somewhat larger for *some* seats. The dominant party does not change appreciably the average partisanship of it's own incumbents' districts, but it does pack the opposition somewhat, by increasing the average partisanship of the out party's incumbents by about two percentage points. When control is divided the changes in district partisanship are quite small, and only Republican incumbents appear to benefit. On average, Democratic incumbents lose slightly. Thus, the changes in district partisanship are not consistent with bipartisan gerrymandering along the lines alleged in the Connecticut redistricting challenged under *Gaffney v. Cummings*. If anything, they are more consistent with claims of partisan gerrymandering.

This is not to say that instances of bipartisan gerrymandering never arise. But, even under divided government, such cases are evidently exceptions, not the norm. To the extent there is any evidence that gerrymandering affects the underlying partisanship of districts it is of old-fashioned variety--packing the out-party--rather than duopolistic, cartel-like division of the electoral terrain "to preserve status quo ante." Even these effects, however, are quite small and not systematic.

3.2. Changing personal votes

An alternate line of thinking holds that redistricting might increase the personal vote for the representative. One view of the incumbency effect holds that ***498** it reflects personal support in the district arising through such factors as name recognition, constituency service, and a voting record tailored to local voters' preferences (Cain, Ferejohn, and Fiorina, 1987). Redistricting might increase the personal vote of incumbents by shaving off precincts where the incumbent has the weakest personal support.

Changes in districts have been used in past studies to capture the extent of the personal vote. Following redistrictingmany districts consist of areas that the legislator previously represented and areas that the legislator had not represented in the past--old voters and new voters. Controlling for the partisanship of the areas, the difference in the incumbent's vote share between old areas and new areas in the election following redistricting will reflect the extent of the personal vote (see, e.g., Fiorina, 1974; Rush, 1993; Ansolabehere, Snyder, and Stewart, 2000). As Ansolabehere, Snyder, and Stewart (2000) note, when new areas are added to an incumbent's district, the incumbent will initially have far less name recognition in the new area, and fewer voters in the new area will know about the incumbent's prior constituency service, voting record, etc. [14] From the incumbents' point of view, these new areas are unknown, potentially hostile areas that require extra attention and investment to win over.

Analysis of the difference in vote between old voters and new voters suggests that the personal vote accounts for approximately half of the incumbency effect. The overall incumbency effect in a typical U.S. House race is approximately 8 to 10 percent and the estimate of the personal vote from the analysis of old and new areas is approximately 4 to 5 percent. [15]

The comparison of old voters and new voters suggest that redistricting itself hurts incumbents to the extent that it changes their constituencies, thereby lowering the size of the personal vote. That fact, combined with the result that gains in partisan support tend to be small, suggests that redistricting itself tends to lower the normal avenues through which incumbents might gain electorally--that is through the partisan vote and the personal vote. Rearranging election boundaries, then, undermines the incumbency advantage at the micro-level.We turn next to the macro-level.

4. COMPARING THE U.S. HOUSE WITH OTHER OFFICES

The comparison of elections across episodes of redistricting suggests that redistricting tends to lower levels of electoral support and loyalty. This suggests that the incumbency effect overall is muted, not inflated, by redistricting. Another way to check this is to compare offices that are districted with those those that are not. Statewide offices-U.S. senator, governor, attorney general, secretary of state, treasurer, and so on--are never redistricted. If redistricting were the main cause of the incumbency advantage, we would expect these offices to exhibit a small incumbency effect, or none at all.

In fact, the incumbency advantage is a general phenomenon, not one limited to offices chosen from districts. While the incumbency advantage was first noted in U.S. House elections, early studies also documented its presence in U.S. Senate elections and gubernatorial elections--offices that are not districted. Subsequent studies have found that the incumbency advantage exists in nearly every elected state office in the United States (e.g., Ansolabehere and Snyder, 2002). Moreover, incumbency advantages rose in every office from 1940 to the present, including those not districted. The incumbency advantages for offices ranging from governor and U.S. senator to state auditor and state commissioners rose at the same time and at the same rate. In the 1940s all offices had relatively small incumbency advantages, of about two percentage points. During the 1950s, those advantages began to grow and their growth accelerated in the 1960s, reaching about seven percent by 1970. The incumbency advantage has continued to creep up, rising to about 10 percent in the 1980s and 1990s. The accelerated growth of

the incumbency advantage during the 1960s leads many to point to reapportionment as the culprit. However, those offices not subject to redistricting saw the same pattern of growth. The incumbency advantage is also noticeably larger for these statewide offices than for state legislatures. The coincidence between the rise of the incumbency advantage and the reapportionment cases appears to be just that, a coincidence.

In Fig. 3 we reproduce an updated version of one of the figures in Ansolabehere and Snyder ***499** (2002). It shows the incumbency advantage, measured using the method in Levitt and Wolfram (1997), for U.S. House races, ""higher" statewide offices, and "lower" statewide offices. [16] We estimate the incumbency advantage for each group separately for each decade. The three curves track one another closely, except in the past two decades where the incumbency effect for U.S. House races drops off sharply compared to the higher statewide offices. Redistricting is clearly not the main force driving the growth and current high level of the incumbency effect for the statewide offices, since these are never redistricted.

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FIG. 3. Incumbency advantage for U.S. House races, "higher" statewide offices, and "lower" statewide offices.

Redistricting, then, is not a cause of incumbency effects, nor does it increase vote margins. Instead, redistricting lowers incumbents vote margins, and the more a legislator's district changes, the worse he or she can expect to do at the polls.

5. TURNOVER

Incumbency-centered politics concerns more than just the expected vote received by candidates running for office; it shapes the overall rate of turnover and career lengths of politicians. Arguments that redistricting increases electoral security ultimately turn on the notion that politicians craft constituency boundaries to keep themselves in office, regardless of their electoral margins. Turnover offers the ultimate proof that redistricting on average hurts incumbents.

Consider the U.S. House of Representatives. Let E_t be the percentage of representatives who "exit" in year t (i.e., t is the election year of their last congress). [17] Fig. 4 shows a graph of E_t over time. There are usually large spikes upward during redistricting episodes, which are indicated in the figure with year numbers rather than circles. The figure also reveals the large overall decline in turnover that has occurred in the U.S. House, a phenomenon that has been discussed at length in the congressional literature.

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FIG. 4. Retirement rate of U.S. House of Representatives over time.

A simple regression confirms what the graphs show. Let R_t =1 if t is a year ending in zero and 0 otherwise; so, R denotes the last election year prior to a congressional reapportionment. We also set R_t = 1 for 1964 because of court-ordered redistrictings following the *Baker v. Carr* decision. [18] We regress E on R and also include a third-order polynomial in t to capture trends such as increasing ***500** professionalization and the increasing electoral advantages of incumbency. The results are shown in the first column of Table 4. The estimates imply that the probability of an exit is five percentage points higher in the last year prior to a reapportionment. Since the average exit rate over the period under study was about 20 percent, in relative terms the effect of reapportionment is substantial--on the order of 25 percent.

TABLE 4. LEGISLATIVE EXIT RATES

	U.S. House	*State Legislatures*
Reapp/Redist	5.14 [a1] (1.87)	5.04 [a1] (1.63)
Year	-.19 (.27)	-.20 (.43)
Year2	-.00 (.01)	-.01 (.02)

Year3	00 (.00)	00 (.00)
Constant	28.14 (2.82)	42.48 (2.46)
N	47	25
R2	.52	.78

Dependent variable = *E*. Independent variable Reapp/Redist = *R*. See text for details.
Standard errors in parentheses.

Footnotes

a1 = statistically significant at the .05 level.

Figure 5 shows an analogous graph for state legislatures, aggregating across both upper and lower chambers. The second column in Table 4 shows the analogous regression. The estimated effect of redistricting is similar to that for the U.S. House, about five percentage points. Note also that the effects of redistricting are clearest in the post*Baker* period. [19] Note also that, as in the case of the U.S. House, average turnover in state legislatures has dropped sharply over time.

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FIG. 5. Retirement rate of state representatives over time.

6. CONCLUSIONS

Redistricting has long been thought to create the conditions that favor incumbents in elections. That line of thinking, we find, lacks empirical support. Redistricting actually lowers incumbency advantages and vote margins and increases turnover, rather than the other way around. We have further focused on one important, recent line of thinking about the districting process--namely that the current process protects incumbents through widespread bipartisan gerrymanders that have made districts less responsive and have even produced the incumbency advantage. A closer look at the key empirical indicators of electoral competition-including the number of competitive districts, the degree of bimodality or spread in the distribution of district partisanship, and the effects of the districting process on the partisanship of incumbents' districts--reveals little or no evidence consistent with the notion that incumbents systematically engage in bipartisan gerrymanders or that bipartisan gerrymanders have created a bimodal or skewed distribution of district partisanship. This is not to say that individual legislators and parties do not try mightily to shape the districts in their favor. They surely do. Rather, the evidence presented here suggests that incumbency protection is difficult to accomplish. There may be some districts successfully drawn to protect incumbents, but the overall picture of legislative districting does not bear that out as a systematic consequence of the redistricting process.

We might hope that by presenting a large number of empirical problems with the argument, we might succeed in putting it to rest. This would probably be overly optimistic, since we are not the first to disconfirm the argument. In the 1970s, influential papers by Ferejohn (1977) and others showed no link between these two phenomena. Several papers in the 1980s also looked for the smoking gun that redistricting inflated the incumbency effect, but found none. Even the best evidence mustered by advocates of a connection between redistricting and incumbency accounts for only about a two percent effect and, then only under the right circumstances. The overall incumbency effect is usually ***501** estimated to be in the range of 8 to 10 percentage points.

The link between redistricting and incumbency advantages has strong intuitive appeal. The argument gained currency in the mid 1970s shortly after the redistricting revolution of the 1960s and the observation of rising incumbency advantages in the same period. Indeed, every 10 years or so, the idea resurfaces. Most recently, Shotts (2001, 2002) and Cox and Katz (2002) provide game theoretic models, supported by some empirical evidence, showing how politicians can manipulate boundaries given the constraints imposed by the courts. Most troubling, the argument seems to have found new adherents in the debate over polarization, and it is widely believed among casual observers of contemporary U.S. politics that the structure of districts contributes to the dissensus in the country. It cannot--the distribution of district preferences is unimodal in its underlying partisanship, not bimodal.

Why this argument lives on despite repeated empirical studies to the contrary owes, we think, to three reasons. First, the coincidence is just too good. Both phenomena arose in the 1960s; could that really have been an accident? We think the answer is yes. Aldrich and Niemi (1996) describe both phenomena as part of a culture of dealignment, but see no link between them. Second, every 10 years every American state engages in bitter battles over new legislative district boundaries. These fights expose the deep interests that sitting legislators have in reinforcing their electoral security. The evidence mounted here suggests that what is really exposed is the fear associated with losing seats that are otherwise secure. Third, political science as yet has no compelling explanation for the incumbency advantage. In the absence of such an explanation, any and every story, including those inconsistent with basic facts, inexorably live on.

We are not arguing that legislative control of the redistricting process is without problems. Quite the contrary. In the age before *Baker v. Carr*, legislatures had complete control over the process. In that era, malapportionment and other maladies protected the *status quo ante*, often by not shifting district lines at all. Today, thanks to the interventions of the courts since the 1960s, legislators are forced to draw new district lines on a regular decennial schedule. The current process has been roundly criticized for allowing legislators to skew the process. The potential for legislators to use redistricting to benefit themselves or their party is always present. However, since the 1960s, the primacy of the state legislatures in the districting process has been greatly reduced, and their authority is regularly challenged in the courts and at the ballot.

The districting process itself continues to evolve. While most states still allow their state legislatures to draw the lines, several states departed from politics as usual and experimented with commissions and other rules in 2012. California enacted a new commission, designed with firewalls against influence by the legislature. State legislators and members of Congress were not even allowed to testify at the commission's proceedings. Arizona, Washington, Idaho, and New Jersey have similarly strong commissions. Florida amended its constitution to impose a new set of criteria, including provisions that prohibit favoring or disfavoring any incumbent or party. And, the legal wrangling over districting is as intense as ever. By Justin Levitt's count, courts heard 190 lawsuits concerning redistricting in 25 different states. At the time of this writing with the 2012 election two months away, 69 of those suits were still active, including suits in the most populous states (California, Texas, New York, Florida, Illinois, and Pennsylvania). [20] Radically new procedures in states like California, Arizona, and Florida, and widespread legal challenges to legislative district plans will make 2012 a critical year not only for the composition of American legislatures, but for our understanding of how the districting process and the districting plans affect representation and electoral competition. The simple prediction from our analysis is that there will not be a significant increase in incumbent reelection rates or vote margins. Rather, we expect 2012 to be one of the most difficult elections for sitting legislators in recent history.

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1 For a history of that case and its consequences, see Ansolabehere and Snyder (2008).

2 Ibid., page 187.

3 See especially, Issacharoff, op. cit. page 598. See also, Cox and Katz (2002). For other arguments, see, e.g., Coate and Knight (2007).

4 See Butler and Cain (1992) for a discussion of these distinctions.

5 Alan Ehrenhalt, "Frankfurter's Curse," *Governing*, January 2004 < http://www.governing.com>.

6 On its own, Persily's criticism may not hold much force, as one might argue that legislators' involvement in redistricting prevented their districts from becoming even worse. However, the fact that commissions and courts produce very similar results to legislature-drawn maps suggests that incumbents have relatively little ability to control their own fates through the redistricting process.

7 Issacharoff, op. cit., page 598.

8 Pooling all of the states into a single graph might obscure the true pattern. The figures in the appendix of Ansolabehere and Snyder (2008) show that this is not the case. They present graphs for all individual legislative chambers. Only one state legislative chamber. the New York State Senate, had two distinct humps with the much larger laying in the Republican side of the ledger. About 30 percent of the state's districts are safely Democratic and half are safely Republican. Interestingly, New York's assembly has one hump, not two. A handful of other states had a smaller cluster of safe Democratic seats, likely the product of racial districting requirements. Otherwise, the distribution of votes among the states followed a bellshaped curve.

9 Although the 40% and 60% thresholds are arbitrary, they are commonly used by political scientists to distinguish safe and competitive districts. In any case, using 45% and 55% yields the same substantive conclusion.

10 Many previous studies use the presidential vote to measure district partisanship, including Erikson (1971b), Cantor and Hermson (1997), Ansolabehere, Snyder, and Stewart (2000, 2001), Powell (2000), Jacobson (2004), and Abramowitz, Alexander, and Gunning (2006). It is not perfect, of course, but it is probably the best single measure available at the congressional district level.

11 We use the coding in Appendix Table A in Carson and Crespin (n.d).

12 The differences in the first row--for Democratic incumbents in states with two or more districts--is significant at the .10 level.

13 From the *Almanac of American Politics, 1994*, page 651.

14 McKee (2008) finds lower levels of name recognition of House incumbents among survey respondents in areas that are redrawn during districting.

15 See Ansolabehere, Snyder, and Stewart (2000). There is some evidence that the incumbency effect is not linear, being smaller in districts that are overwhelmingly of one party (Krashensky and Milne, 1993).

16 Higher statewide offices are U.S. senator, governor, lieutenant governor, attorney general, and secretary of state. The lower offices are treasurer, auditor, controller, comptroller, school superintendent, commissioner of agriculture, commissioner of corporations, commissioner of public utilities, commissioner of insurance, commissioner of mines, commissioner of labor, and commissioner of lands.

17 We begin the analysis in 1912 because the size of the House has been constant at 435 since then, except for 1959-1962 when the number of representatives was temporarily expanded to 437 following the admission of Alaska and Hawaii as states. We adjust accordingly in computing E_t. If a representative has non-contiguous periods of service, he or she will have multiple exits, one for each period.

18 In 1964 in *Wesberry v. Sanders*, the Supreme Court held that all congressional districts must adhere to the one-person-onevote standard. Almost all states redistricted during the second half of the 1960s, some more than once.

19 In 1964 in *Reynolds v. Sims*, the Supreme Court held that all state legislative districts must adhere to the one-person-onevote standard.

20 <http://redistricting.lls.edu/cases.php>.

Exhibit E

Exhibit E

Asking About Numbers: Why and How*

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Abstract

Survey questions about quantities offer a number of advantages over more common qualitative questions. However, concerns about survey respondents' abilities to accurately report numbers have limited the use of quantitative questions. This paper shows quantitative questions are feasible and useful for the study of economic voting. First, survey respondents are capable of accurately assessing familiar economic quantities, such as the price of gas. Second, careful question design—in particular providing respondents with benchmark quantities—can reduce measurement error due to respondents not understanding the scale on which more complex quantities, such as the unemployment rate, are measured. Third, combining quantitative and qualitative questions sheds light on where partisan bias enters economic assessments: in perceiving, judging, or reporting economic quantities.

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1 Introduction

Knowledge of the economy is important to a broad range of decisions: from private decisions, like investment and educational choices, to public decisions, such as voting for bond issues or holding the President accountable for his economic policy choices. Economic information is often expressed in numbers, for example, prices, rates, and even consumer confidence. Yet there is very little public opinion research on what citizens know about economic quantities, and how they use that knowledge to make decisions.[1]

This lack of research is perplexing, as theories of economic voting are fundamentally rooted in numbers. In particular, the political economy literature focuses on how vote shares change with economic quantities such as GDP growth, the unemployment rate, or inflation (e.g. Kramer (1971); Fair (1978); Alesina, Londregan and Rosenthal (1993)). Parallel investigations by survey researchers, however, have not yielded a set of findings that can be readily linked to statistical models used in analyses of such aggregate data.[2] A fundamental cause of this disconnect is that survey researchers tend to rely on respondents' qualitative evaluations of the economy, rather than ask about quantities directly. Thus, developing standard questions to measure perceptions of economic quantities is essential for testing theories of economic voting. Our goal is to take a first step towards developing such standard questions by evaluating survey questions that directly ask about quantities.[3]

This paper shows that survey respondents provide accurate responses to questions about familiar quantities, such as the price of gas. Moreover, we show that careful question design can reduce problems associated with quantitative questions when asking about more complex quantities, such as the unemployment rate. Finally, we show that quantitative questions

[1] Quantities are relevant for a wide range of policy areas: for example: average test scores, incarceration rates, abortion rates, and healthcare expenditures.

[2] For example, whether voting is based on aggregate economic outcomes or a voter's personal economic outcomes (whether voters are sociotropic or egotropic, see Kinder and Kiewiet, 1979; 1981) does not affect patterns in aggregate data because, on average, personal outcomes improve when aggregate outcomes improve.

[3] In other domains quantitative questions may be useful for measuring exposure or knowledge rather than perceptions. As our findings focus on the measurement of perceptions, more research is needed to understand how quantitative questions function in measuring these other dimensions.

seem to exhibit less partisan bias than qualitative questions. That is, this paper summarizes the advantages of, and concerns about, quantitative questions, and makes some progress in alleviating those concerns.

For the past 30 years, the standard survey question used to measure economic evaluations and study economic voting has been some variant of the retrospective economic evaluation:

> Now thinking about the economy in the country as a whole, would you say that over the past year, the nation's economy has gotten better, stayed about the same, or gotten worse?

There are a number of advantages to such *qualitative questions*. No sophisticated knowledge of economics is required to understand or respond to this question. It is also broad, allowing researchers to measure economic evaluations with a single question. Finally, this question is easily portable across surveys, facilitating comparisons across both time and space about the relationship between economic assessments and political behavior.

These advantages, however, come with some costs. The retrospective economic evaluation does not relate to a specific dimension of the economy—for example, GDP growth, levels of employment, tax policy, or changes in prices (inflation). Such vagueness invites projection of attitudes other than economic understanding, such as political beliefs. It also reduces complex economic assessments to one of three possible answers.[4] Finally, this question cannot separate a respondent's perception of economic conditions on several dimensions from his or her judgment of whether those perceived conditions represent an improvement.

While *quantitative questions* are not perfect, they have a distinct conceptual advantage over qualitative questions. Namely, they can be used to ask respondents about the basic building blocks of many theories: quantities. However, quantitative questions remain largely unused in the study of economic voting due to concerns about whether respondents are capable of understanding and reporting economic quantities. Previous work generally finds

[4]Many election surveys also let respondents also choose between "somewhat" and "substantially" better and worse.

that a substantial number of people report wildly inaccurate views about quantities such as the unemployment rate or inflation (Conover, Feldman and Knight, 1986; Holbrook and Garand, 1996; Blendon et al., 1997; Curtin, 2007). However, if a majority of voters are indeed unable to understand economic quantities, this calls into question the assumptions on which most political economic models are built.

In order to show respondents can handle numbers, we examine their assessments of the price of gas. Gasoline expenditures are a sizable portion of most households' budgets, and the price of gas is frequently encountered in respondents' everyday environment. Thus, inaccurate reports of the price of gas would suggest there are few economic quantities respondents can accurately report. However, in three different election surveys run since 2006, individuals accurately report the average price of gas. Moreover, there are few observable characteristics that consistently relate to either the bias or accuracy of reported gas prices. One characteristic that does make individuals more accurate is exposure to gas prices—for example, through driving—which is consistent with the idea that survey respondents are capable of reporting quantities with which they are familiar.

Asking about more complex quantities requires more care. In particular, we advocate the use of benchmarks to describe the scale on which the quantity of interest is measured. For example, most respondents are infrequently exposed to the unemployment rate, if at all, and thus provide very inaccurate assessments. A potential reason that is discussed in the literature for this inaccuracy is that not all respondents share a common understanding of what is meant by the unemployment rate, and hence that they are not reporting unemployment rates on the same scale. We show that providing respondents with the historical range and average level of the unemployment rate reduces differences in reported rates between higher and lower socioeconomic-status respondents. This is consistent with benchmarks reducing inaccurate responses caused by differential understandings of the scale on which economic quantities are measured.

In contrast to reported gas prices, many observable characteristics systematically as-

sociate with respondents' reports of the unemployment rate. Consistent with the mecro-economic voting theory of Ansolabehere, Meredith and Snowberg (2011*a*), we show in four election surveys in three different years that observable characteristics associated with a higher risk of unemployment increase reported unemployment rates. Moreover, reported unemployment rates rose in lockstep with the actual unemployment rate between 2008 and 2009.

Finally, we demonstrate that quantitative questions can help us to better understand the sources of partisan differences in responses to qualitative questions (Wlezien, Franklin and Twiggs, 1997; Anderson, Mendes and Tverdova, 2004; Evans and Andersen, 2006; Evans and Pickup, 2010). Combining information about economic perceptions with qualitative evaluations allows us to infer the criteria respondents use to judge economic performance. Our results suggest that partisan differences result from biases in the reporting of economic assessments, whether quantitative or qualitative. Moreover, quantitative questions evince less partisan bias than corresponding qualitative questions.

2 Why Ask About Quantities?

It is perhaps surprising that public opinion research focuses on qualitative—rather than quantitative—survey questions, given that many theories are rooted in numbers. For example, the theory of economic voting, and aggregate level studies about it, primarily focus on how political support for the incumbent party changes with various economic quantities, such as GDP growth, the unemployment rate, and inflation (Kramer, 1971; MacKuen, Erikson and Stimson, 1992; Erikson, MacKuen and Stimson, 2002; van der Brug, van der Eijk and Franklin, 2007). However, public opinion research into the individual-level mechanisms of economic voting has focused on the qualitative, retrospective economic evaluation discussed in the introduction. Supporting this focus are concerns that quantitative questions are too cognitively difficult for survey respondents.

This section details the potential advantages of quantitative questions. We then explore the concerns that have prevented a wider adoption of quantitative questions, and briefly preview how our research addresses these concerns through the careful design of quantitative questions.

2.1 Benefits of Quantitative Questions

Quantitative questions have four main advantages over qualitative questions. Quantitative questions allow researchers to separate respondents' perceptions of conditions from their judgments of those perceptions; they allow open responses; allow perceptions to be compared with actual conditions; and place respondents' responses on a standard scale.

Theories of economic voting posit that voters form *perceptions* of economic performance, and then *judge* these perceptions relative to some benchmark.[5] Quantitative questions allow researchers to separately elicit information about these two steps. Without good measures of economic perceptions, researchers risk both incorrectly calculating the amount of economic voting, and a poor understanding of the mechanisms underlying it (Kramer, 1983; Wlezien, Franklin and Twiggs, 1997; Anderson, Mendes and Tverdova, 2004; Evans and Andersen, 2006; Evans and Pickup, 2010). Moreover, evaluations of complex issues, like the economy, are often multi-dimensional (Zaller and Feldman, 1992). As economic quantities are very specific, quantitative questions allow researchers to measure perceptions of the economy on multiple dimensions, and, in combination with a respondent's overall, qualitative evaluation, understand how these multiple perceptions are judged.

A second advantage of quantitative questions is that response is *open*—that is, response options can be relatively unconstrained. In contrast, qualitative questions are often *closed*—that is, constrained—to keep responses relevant and to reduce the cost of coding answers (Schuman and Presser, 1981).[6] Open questions offer two advantages over closed questions.

[5]The benchmark may be a fixed standard in retrospective voting models, or depend on perceptions of opposition candidates in prospective models.

[6]It is possible to construct hybrid questions that put verbal labels on specific quantities or ranges of

First, responses to closed questions are affected by the presentation and choice of response categories (Schwarz et al., 1985; Rockwood, Sangster and Dillman, 1997). Second, closed questions limit the amount of variation in responses (Groves et al., 2004). For example, individuals who perceive the unemployment rate to be 7% and 10%, respectively, may both report that the economy has "gotten worse", even though those who think it is 10% believe there is almost 50% more unemployment. In practice this may result in a large loss of variation: on the 2008 American National Election Survey (ANES), 90% of respondents reported that the economy had "gotten worse". Increasing the amount of variation captured by survey responses is particularly useful in understanding how economic information affects economic perceptions (Manski, 2004).[7]

Third, as quantitative questions often have an objectively correct answer, researchers can compare respondents' answers with actual conditions. For example, Hetherington (1996) shows that more media consumption is associated with worse economic evaluations. However, this study could not determine which group is more or less accurate in its perceptions.[8] This contrasts with Ansolabehere, Snowberg and Snyder (2005) which uses quantitative questions to show that low media consumption is associated with more accurate perceptions of the amount of money spent in congressional campaigns.

A fourth advantage of quantitative questions is that they facilitate comparisons across respondents. Survey respondents are likely to differ in their interpretations of what is meant by verbal phrases (Beyth-Marom, 1982; Wallsten, 1986; Wright, Gaskell and O'Muircheartaigh, 1994). For example, research shows wide variation in the numerical interpretations of various verbal expressions of uncertainty: the probability assigned to the term "likely" varies from 42% at the 10th percentile to 81% at the 90th percentile (Beyth-Marom, 1982). Such

quantities. See Juster (1966) for an example.

[7]This potential variation, however, may not be fully utilized as respondents tend to choose focal numbers, like multiples of five, when reporting quantities (Tourangeau and Smith, 1996; Tourangeau et al., 1997).

[8]Qualitative questions are occasionally used to identify inaccurate perceptions. For example, Bartels (2002) shows that a substantial percentage of respondents on the 1988 ANES report that inflation got worse over the course of the Reagan administration, when, objectively, inflation was significantly lower at the end of Reagan's term than at the beginning.

interpretation differences may cause two individuals with identical underlying perceptions to give different answers to qualitative questions. In contrast, quantities like gas prices and the unemployment rate are measured on a well defined scale.

2.2 Can Survey Respondents Handle Numbers?

Despite these potential advantages, quantitative questions are infrequently used on political surveys.[9] The focus on qualitative questions comes largely from concerns that quantitative questions are too cognitively complex for survey respondents. This concern manifests itself in a number of ways: from a general perception that survey respondents cannot "handle" or do not like quantitative questions, to more specific mechanisms about how these cognitive costs create biases in responses. Additionally, the fact that quantitative questions put responses on a common scale can turn from asset to hindrance if respondents have different understandings of that scale.[10]

General concerns that quantitative questions are too cognitively demanding are found in research showing that survey respondents report wildly inaccurate assessments of quantities such as the population of minority groups, or the percentage of the federal budget spent on welfare and foreign aid (Nadeau, Niemi and Levine, 1993; Kuklinski et al., 2000; Gilens, 2001). Moreover, respondents indicate a preference for communicating numerical values qualitatively rather than quantitatively, although there is some heterogeneity (Erev and Cohen, 1990; Wallsten et al., 1993). Together, these findings question individuals' abilities to accurately report numbers (see Moxey and Sanford, 2000 for a summary).

Even if respondents are capable of providing numerical assessments, there are concerns that they may not have the motivation to do so. While motivation is a general problem in survey research, two factors make the problem more acute when asking about quantities. First, *survey satisficing*—the behavioral pattern of providing acceptable, rather than

[9]Quantitative questions are more frequently used on economic surveys. For example, the Michigan Survey of Consumer Behavior asks both quantitative and qualitative questions about inflation expectations.

[10]We advocate the use of benchmarks to put respondents on a common scale; see Section 4.

optimal, answers—increases when assessments are difficult to perform and report (Krosnick, 1991). Moreover, some respondents feel compelled to provide answers to factual questions, rather than stating they "don't know" (Nadeau and Niemi, 1995). If these respondents lack the motivation to produce responses that accurately reflect their perceptions, then these additional answers may introduce unwanted noise.[11] For example, Bruine de Bruin et al. (2000) find that survey respondents reporting a probability of 0.5 are often expressing general uncertainty, rather than a belief of a 50% chance.

Despite these concerns, we find in Section 3 that when respondents are asked about quantities they have direct experience with, such as gas prices, their responses are extremely accurate. This contradicts the hypothesis that survey respondents are generally unable to report numbers, either because they are incapable or because they lack motivation. Yet, there still may be concerns that certain types of individuals, like those with less education, may find it difficult to accurately report quantities. However, few covariates systematically associate with the accuracy of gas prices assessments. Thus, there appears to be widespread ability to accurately report familiar economic quantities.

2.3 Do Respondents Really use a Common Scale?

An advantage of quantitative questions is that they may put survey responses on a common scale. A common scale is useful as it allows researchers to interpret higher reported unemployment rates as higher perceptions of unemployment.[12] However, this interpretation may not be valid if respondents are unfamiliar with the scale on which a quantity is measured. Indeed, 23%, 34%, and 40% of nationally representative sample report that they have never heard of the unemployment rate, Consumer Price Index, and Gross Domestic

[11] On the other hand, if quantitative questions motivate respondents to produce accurate responses because they know there is a wrong answer, this may be an added benefit of quantitative questions. Bullock, Gerber and Huber (2010) finds that providing financial incentives reduces, but does not eliminate, partisan differences in reported perceptions of facts.

[12] Moreover, if subjects perceive a scale linearly, then those that report 10% unemployment can be interpreted as perceiving twice as much unemployment as those that report 5% unemployment. However, as none of our discussion requires a linear scale, we discuss our results in terms of the weaker condition of a monotonic, but not necessarily linear, common scale.

Product, respectively (Curtin, 2007). Moreover, the percentage of low socio-economic status (SES) respondents who do not know these quantities is substantially larger.[13] This leads to concerns that heterogeneity in respondents' reports of technical economic quantities may be due to different levels of knowledge about the definition of those quantities, rather than different perceptions of the conditions that underly those quantities.

Although Section 4 shows this concern is valid, the same section also shows that careful question design produces patterns consistent with respondents using a common scale. In particular, we show that by providing benchmarks in questions about quantities—in this case, information on the historical tendencies of the unemployment rate—reduces both the number of outlying responses, and differences between the reports of higher and lower socio-economic status respondents. This is consistent with benchmarks producing a common scale for all respondents.

A final concern is whether there is useful information in respondents' assessments of quantities. While this is a tricky question to answer, we present a variety of evidence demonstrating that reported economic quantities do reflect perceptions of the economy. First, changes in gas prices and the unemployment rate are reflected in changes in quantitative assessments over time. Second, the strong relationship between qualitative and quantitative economic assessments suggests responses to both are driven by common underlying factors. Third, and finally, consistent with the mecro-economic voting theory of Ansolabehere, Meredith and Snowberg (2011*a*), cross-sectional variation in reported unemployment rates reflects differences in respondents' probability of being unemployed.

3 Survey Respondents Can Handle Numbers

The previous section highlights some skepticism about respondents' abilities to report perceptions of *any* numerical quantity. To address this skepticism, we demonstrate that respondents

[13]For example, the percentage of people at the bottom, middle, and top tercile of the income distribution who have not heard of the unemployment rate is 38%, 21%, and 11%, respectively.

can accurately report the average price of gas in their state.[14] We focus on the price of gas because it one of the most frequently encountered numbers in a respondent's everyday environment. Gasoline is also an important component of U.S. household consumption and gas prices have an independent effect on presidential approval (Fox, 2009).[15] Finally, actual gas prices are easy to observe, which facilitates comparison between respondents' assessments and actual prices.

As gas prices are salient and well understood by most people, we expect that perceptions will be quite accurate. Observing inaccurate assessments of gas prices would therefore suggest that there are few or no economic quantities that respondents can accurately report. In contrast, if individuals can accurately report the price of gas, then inaccurate assessments of other economic quantities is not simply a reflection of general innumeracy.

We asked respondents the following question on the 2006 and 2008 American National Election Survey (ANES), and on the 2008 Cooperative Congressional Election Survey (CCES):

> What is your best guess of the average price of a gallon of regular unleaded gasoline across all of [YOUR STATE] today?

Respondents accurately report the average price of gas in their state of residence. Figure 1 plots a histogram of the reported price of gas in each of the three surveys. We observe that the modal response in each survey tracks the actual average price of gas. It is particularly impressive that the large drop in gas prices between October and November 2008 is quickly reflected in survey responses.

Figure 2 plots the difference between respondents' reported price of gas and the actual average price of gas in the respondents' state in the month of the survey. This difference is distributed relatively evenly around a mean of about zero. On all three surveys, the average *bias*—the difference between the reported and actual price of gas—is slightly positive.[16] The

[14] All of the data files to replicate our analysis can be found in Ansolabehere, Meredith and Snowberg (2012).

[15] U.S. Bureau of Labor Statistics news release USDL-10-1390 indicates that gasoline and motor oil account for about five percent of American household expenditures between 2007 and 2009.

[16] We bottom-code and top-code the bias at -$1.00 and $1.00 respectively.

mean response overestimates the price of gas by approximately ten cents on all three surveys, while the median response overestimates the price by between six to ten cents.

Overall, the accuracy of assessments of gas prices show survey respondents are capable of accurately answering quantitative questions about a familiar quantity. The *accuracy* of responses—the negative of the absolute difference between the reported and actual price of gas—is −20 to −30 cents across the three surveys, with the median deviation being slightly lower.[17] Given that the average price of gas in the sample ranges from two to three dollars, this suggests a 10% to 15% average difference between the reported and actual price of gas.

The above analysis shows respondents are accurate, on average. However, there may be considerable heterogeneity in respondents' abilities to perceive and report numerical quantities. For example, more education may help respondents report more accurate numeric assessments. Table 1 explores this possibility by using a multi-variate regression to determine the correlates of bias and accuracy of reported gas prices in all three surveys.

The table shows that different types of people do not systematically differ in their ability to accurately answer questions about a familiar quantity. There are few covariates that are consistent predictors of either the bias or accuracy of assessments of the price of gas. Moreover, most of the coefficients are both small in magnitude and insignificant. There are two exceptions to this general pattern: Black and Hispanic respondents are significantly less accurate than non-Black, non-Hispanic respondents. Additionally, in contrast to findings in the next section about reported unemployment rates, there is little partisan difference in assessments of the price of gas.

Finally, although education is not significantly correlated with the accuracy of gas price assessments, other activities that increase a respondent's familiarity with a numerical quantity are. The 2006 ANES asked respondents both how many times a week they drove a car, and how often they noticed gas prices. Each additional day that a respondent reports

[17]Note that as accuracy is defined as the negative of the absolute difference, accuracy increases as the absolute difference between a respondent's report and the actual price decreases. Gasoline price data is from the U.S. Department of Transportation.

driving makes them 0.8 cents (s.e. = 0.4) more accurate, while each additional time that a respondent reports noticing gas prices makes them 1.6 cents (s.e. = 0.4) more accurate (Ansolabehere, Meredith and Snowberg, 2011*b*). These point estimates suggest that everyday that a respondent drives and notices gas prices is associated with 12.5% improvement in his or her accuracy. This examination would not be possible with qualitative questions.

4 The Benefits of Benchmarking

Understanding individuals' perceptions of economic quantities is potentially quite useful for understanding the mechanisms underlying economic voting. However, as outlined in Section 2.3, many respondents are unfamiliar with complicated quantities like the unemployment rate, and the scale on which those quantities are measured. As a result, individuals may report their perceptions of the percentage of people who are not working when asked about their perceptions of the unemployment rate. Thus, heterogeneity in responses may be driven by heterogeneity in individuals' understanding of what the unemployment rate is meant to measure. While such heterogeneity may be useful for measuring familiarity or knowledge of the unemployment rate, it is problematic to use these assessments to examine how perceptions of labor market conditions affect voter behavior.

This section advocates the use of benchmarks when asking about complex economic quantities. Benchmarks refer to reference values of the quantity in question that are embedded in the question wording, and ideally provide a sense of scale of the quantity to those who do not know it, without changing the perceptions of those who do.

We investigate the effect of including benchmarks when asking the following question, asked on the 2006 and 2008 ANES, and the 2008 and 2009 CCES:

> As far as you know, what is the current rate of unemployment? That is, of the adults in the US who wanted to work during the second week of October, what percent of them would you guess were unemployed and looking for a job?

The following benchmarks were sometimes added to the above question on the 2008 and 2009 CCES:

> The unemployment rate in the U.S. has varied between 2.5% and 10.8% between 1948 and today. The average unemployment rate during that time was 5.8%.

Comparing the responses of those who were exposed and not exposed to the benchmarks shows that including benchmarks reduces heterogeneity in reported unemployment rates, especially among low SES respondents. Yet, adding these benchmarks does not reduce meaningful variation in reported unemployment rates. We show these results in a randomized experiment with approximately 4,000 respondents, administered as part of the 2008 CCES. Three-quarters of these respondents were asked about the unemployment rate with a question that included the benchmarks, and the remaining one-quarter were asked the same question without them. We refer to these as the benchmarked and non-benchmarked samples, respectively.[18]

4.1 A Simple Model of Survey Response

The lack of familiarity with complicated economic quantities, like the unemployment rate, raises questions about whether or not assessments of these quantities are on a common scale. For example, respondents who are not familiar with the technical definition of the unemployment rate may report their perception of the percent of people out of work, rather than those that are out of work *and* looking for a job. That is, they may report their perception of the labor force non-participation rate, rather than their perception of the unemployment rate.[19] Thus, heterogeneity in assessments of the unemployment rate may primarily reflect

[18] Due to an error administrating the survey, the benchmarked sample allowed respondents to enter unemployment rates to the tenth of a percent, while the unframed sample was only able to enter them to the nearest percent. Truncating or rounding the responses in the benchmarked sample has almost no effect on the results presented here, although we cannot rule out that this biased responses by serving as a cue about the expected precision of response.

[19] A large literature also shows that individuals struggle to estimate low probabilities, like the probability that people are unemployed (Kahneman and Tversky, 1979; Snowberg and Wolfers, 2010).

heterogeneity in definitions and scaling, rather than heterogeneity in perceptions of unemployment.

A well-designed question would substantially reduce heterogeneity in responses due to different understandings of the quantity of interest, without reducing meaningful variation.[20] To ascertain whether including our benchmarks satisfy these goals requires a theory of what, exactly, constitutes meaningful variation, and which responses are due to lack of a common scale. Meaningful variation is easy to define: it is variation due to differences in perceptions of the economic concept underlying the quantity. As this cannot be measured directly, the rest of this section builds a simple model of survey response that makes predictions about what would occur if a question's design reduces heterogeneity due to non-common scaling, while maintaining meaningful variation.

We assume there are three types of survey respondents: those who know the scale on which the unemployment rate is measured, those who do not know the scale, and those who are not attempting to correctly answer the question. Benchmarks would ideally provide a sense of scale to those who do not know it, without reducing the meaningful variation among those who do.

This simple structure is enough to deduce two patterns that we should observe if our question is well-designed. The first pattern is that more information about the definition and tendencies of the unemployment rate should reduce the proportion of respondents reporting very large unemployment rates (say, greater than 15%). In particular, including the precise definition of the unemployment rate should produce an initial reduction, and the inclusion of benchmarks a further reduction.

Second, there should be a similar relationship between reported unemployment and qualitative economic evaluations among those who report an unemployment rate within the

[20] In a model of on-the-spot survey response (Converse, 1964; Zaller, 1992), a well-designed question would provide information that would not influence a respondents perception of the economy, but instead help them translate that perception into a common numeric scale. To put this another way, when asking a respondent to rate the President's foreign policy on a scale of 1 to 10, the respondent should not draw any information about foreign policy from the fact that a 1 to 10 scale is used.

historical limits in *both* the benchmarked and non-benchmarked samples. Moreover, as only those who are not attempting to correctly answer the question provide responses greater than the historical maximum in the benchmarked sample, there should not be a significant relationship between reported unemployment rates and qualitative economic evaluations above the historical maximum.

To deduce a third pattern, we maintain an additional two hypotheses that refine the definition of what does—and does not—constitute meaningful variation. The argument in Section 2.3 suggests that low SES respondents will be particularly likely to have a different understanding of the scale on which unemployment is measured. Thus, we assume low SES respondents will be more likely to report a number closer to the labor-force non-participation rate. Maintaining this hypothesis, reductions in the amount of low SES respondents that report very high levels of unemployment should be interpreted as evidence that the benchmarks are reducing heterogeneity due to different understandings of the unemployment rate.

We further maintain that meaningful variation follows the mecro-economic voting hypothesis of Ansolabehere, Meredith and Snowberg (2011*a*). Under this hypothesis, individuals gather local economic information to inform them about their future risks of unemployment. Thus, those who face a greater risk of unemployment should perceive the unemployment rate to be higher.

Taking these two maintained hypotheses together, we deduce a third pattern that should be observed if our benchmarks are well-designed: there should be substantial heterogeneity in the non-benchmarked sample, with those at a greater risk of unemployment and those with low SES providing answers that are, on average, much higher. The benchmarked sample should provide responses consistent with their relative risk of unemployment, but the differences between high and low SES respondents should be substantially reduced.

The next three subsections evaluate the extent to which the above patterns are observed in data from a randomized experiment implemented as part of the 2008 CCES.

4.2 Reduction of Very High Responses

While we believe it is important to precisely define the quantity of interest, it is unlikely that this has much of an effect on survey responses. Compared to previous work, we observe fewer very high responses: only 30% of respondents in the unbenchmarked sample report unemployment rates above the historical high of 10.8%, compared with 50% in previous studies by Conover, Feldman and Knight (1986) and Holbrook and Garand (1996). However, this is likely due to differences in the population being surveyed or the mode of survey, or the changes across time in the salience of unemployment.[21] Assessments above the historical high of 10.8% are particularly concentrated among low SES respondents.

The top panel of Figure 3 suggests that a substantial proportion of the population is roughly aware that 6.5% was the true unemployment rate in October 2008, with just under one-half of the sample reporting unemployment rates of between six and seven percent. However, another one-third of the responses are higher than 10.8%, the highest unemployment rate since the Bureau of Labor Statistics unemployment series began in 1948. This implies either that there are many respondents who believe the unemployment rate is at least twice as high as it actually is, or that many of the extreme answers come from a poor understanding the scale of the unemployment rate.

Next, we add benchmarks to the question. The benchmarks we construct provide the respondent with a sense of the smallest and largest feasible unemployment rates by telling them the historical highs and lows of unemployment during the previous sixty years. It also gives guidance on the average level of unemployment across time.

As shown in the middle panel of Figure 3, providing respondents with benchmarks substantially reduces, but does not eliminate, the heterogeneity in assessments of the unemploy-

[21] In particular, a simple experiment, run on Mechanical Turk, found that the distribution of responses was the same whether or not we provided a definition of the unemployment rate.

We do not provide an opt-out prompt when asking these question, and request that respondents report a number if they try to skip the question. Respondents can opt-out by attempting to skip the question a second time. Our resulting response rates are 99% in the benchmarked and 98% in the non-benchmarked samples, respectively. This protocol is consistent with Curtin's (2007) finding that providing an opt-out significantly reduced responses, without significantly increasing the accuracy of responses.

ment rate. Just as in the non-benchmarked sample, slightly fewer than 50% of respondents report unemployment rates of 6–7%. However, the percentage of answers above the largest historical unemployment rate is reduced from over 30% to about 15% of the sample. Thus, the first pattern conjectured above holds: more information about the definition and tendencies of the unemployment rate reduces the proportion of respondents reporting very large unemployment rates.

4.3 Comparison with Qualitative Evaluations

If respondents view higher unemployment as a sign the economy is getting worse, then there should be a relationship between perceptions of unemployment and qualitative economic evaluations, like the standard retrospective question discussed in the introduction. That is, if reported unemployment rates are capturing variation in unemployment perceptions, we expect that respondents who report higher unemployment rates will report more negative evaluations of the aggregate economy.[22]

This subsection examines how including benchmarks affects the estimated relationship between the reported unemployment rate and retrospective economic evaluations. As noted above, if our simple model of survey response is correct, and the benchmarks are well-designed, then there should be a similar relationship between reported unemployment and qualitative economic evaluations among those who report an unemployment rate within the historical limits in *both* the benchmarked and non-benchmarked samples. Moreover, there should be no relationship between reported unemployment rates and qualitative economic evaluations above the historical maximum in the benchmarked sample.

These predicted patterns are consistent with the data shown in Figure 4. In particular, the top panels show the (Loess smoothed) relationship between respondents' reported unem-

[22]Of course, unemployment perceptions and the threshold at which a respondent will say the economy has gotten worse are not necessarily independent. The answers to these questions could be uncorrelated or negatively correlated if respondents who have a higher threshold also systematically perceive higher levels of unemployment. We view this as an unlikely possibility, and one that is contradicted by the data in the next section.

ployment rate assessments and their qualitative assessment of business conditions and the employment situation, elicited by asking:

> How would you rate the present general business conditions in your area? Good, normal or bad?
>
> What would you say about available jobs in your area right now? There are plenty of available jobs in my area, there are not so many available jobs in my area, or jobs in my area are hard to get?[23]

We code the extremes of "good" and "plenty of jobs" as 1, and "bad" and "hard to get" as 3. All data comes from the 2008 CCES.[24] The bottom two panels show the relationship between respondents' reported unemployment perceptions and their retrospective economic evaluation, and vote choice, respectively.[25]

All four panels are plotted on a log scale to focus on the variation within the historical boundaries. For all three qualitative assessments, the relationship between the qualitative assessment and unemployment rate assessment within that range is quite similar in the benchmarked and non-benchmarked sample. The same pattern holds for vote choice. Statistical tests, contained in the appendix, confirm this appearance. Moreover, in all four panels, there appears to be no relationship between qualitative assessments or vote choice and unemployment rate assessments above the historical maximum in the benchmarked sample.[26]

[23]These are the standard current business condition and employment questions from the Conference Board survey. Note that these questions ask about local economic conditions, as opposed to the retrospective economic evaluation which asks about national economic conditions.

[24] While it would be interesting to compare quantitative and qualitative employment questions in 2009 as well, in that year, 90% of respondents reported jobs were "hard to get". This shows one of the benefits of using quantitative questions: the fact that responses are open means that they measure variation even in extreme circumstances.

[25]The questions were asked in the following sequence: retrospective economic evaluation, business conditions, qualitative employment, vote choice, and unemployment rate. As the unemployment question was asked last, the benchmark could not have affected answers to these other questions. Additional questions were asked before, after, and in between these questions.

[26]One additional result of note is that there no relationship between qualitative assessments or vote choice and unemployment rate assessments above the historical maximum in the *non-benchmarked* sample either. Our model implies that responses above the historical maxmium in the non-benchmarked sample come from a combination of people who don't know the scale on which unemployment is measured and people who are not attempting to answer the question. This pattern thus suggests that those who do not know the scale on which the unemployment rate is measured do not share a common misunderstanding of the scale.

Once again, statistical tests contained in the appendix confirm this appearance.

Thus, the second pattern conjectured above holds: including benchmarks does not affect the relationship between unemployment rate assessments and qualitative assessments in the range defined by historical limits. Moreover, there is no relationship between unemployment rate assessments and qualitative assessments above the historical maximum.

4.4 Mecro-patterns of Unemployment Perceptions

Finally, we examine how introducing the benchmark changes differences in asessments of unemployment between groups. We show that although including benchmarks reduces the heterogeneity between groups, it does not affect the overall pattern of responses: namely, respondents in groups with a higher risk of unemployment report higher average assessments of the unemployment rate. These patterns are thus consistent with the third set of patterns deduced in Section 4.1, and hence, with the question being well-designed under our maintained hypothesis. Moreover, these patterns are substantially different from patterns found in qualitative evaluations, leading to the conclusion that quantitative questions capture additional, meaningful variation.

Data from the benchmarked and non-benchmarked sample is examined in Table 2. Columns 1 and 2 present coefficients from least absolute deviation (LAD) regressions of the reported unemployment rate, in the benchmarked and non-benchmarked sample respectively, on a host of covariates. LAD regressions, sometimes referred to as median regressions, minimize the impact of outliers on estimated coefficients. The coefficients in these regressions report how the median value of reported unemployment associates with a one-unit change in the independent variable, holding all else equal. For example, the median respondent making under $20,000 dollars a year reports unemployment rates that are 0.77 percentage points (s.e. = 0.21) higher in the benchmarked sample, as compared to 13.5 percentage points (s.e. = 5.8) higher in the non-benchmarked sample. This is consistent with the general pattern: the same variables significantly relate to unemployment assessments in both

the benchmarked and non-benchmarked sample. However, the magnitudes are sometimes dramatically different.

Adding benchmarks generally reduces differences between groups in their assessments of unemployment. To perform the comparison, we must first recode each respondent's unemployment assessment as the percentile of the distribution that his or her unemployment assessment falls, *within his or her sample*. This is necessary because, if our theory is correct, respondents in the two samples are implicitly using different scales. Thus, in this coding, 100 indicates that a respondent had the highest report in his or her sample, and 0 the lowest.

Columns 4 and 5 examine how changes in the percentile of respondents' reports (within their sample) are correlated with the same covariates as before. For example, in the non-benchmarked sample, holding all else equal, the average respondent making under $20,000 dollars a year reports a 20.0 percentile (s.e. = 5.8) higher unemployment rate than the average respondent making more than $120,000 dollars a year. In comparison, this shrinks to 10.5 percentiles (s.e. = 2.4) higher in the benchmarked sample. Column 6 reports difference between these estimates is 9.5 percentiles (s.e. = 6.3), although this difference is not statistically significant. Including benchmarks causes statistically significant reductions in the difference in reports between those who never attended college and those with a Bachelor's degree, as well as those with low and high incomes. In addition, we estimate a statistically significant reduction in the difference between married women and unmarried men, who are the least and most likely gender–marriage combinations, respectively, to participate in the labor force. As we conjecture that those with less education, lower incomes, and those outside the labor force are less likely to be aware of the scale on which the unemployment rate is measured, this suggests that our benchmarks help put individuals on a common scale.[27]

[27]There are concerns about the reliability of internet surveys such as the CCES (see Malhotra and Krosnick, 2007 and Yeager et al., 2009, or Stephenson and Crête, 2011 for an opposing view). To alleviate such concerns, we replicate the regressions in Table 2 on responses to a reported unemployment rate question on the 2006 and 2008 ANES in the Appendix. On both surveys, the question did not contain any benchmarks, and the 2006 question differed slightly as it asked respondents their perceptions of the unemployment rate in their state of residence. It is reassuring that the results reported in Table A.2 show very similar patterns in the ANES data to those observed in the CCES data. For more information on response rates and data reliability in the 2008 CCES, see Ansolabehere (2011), especially the guide.

A final question of interest here is whether we could have observed the same mecro-economic patterns using only qualitative assessments. To determine this, in Table 3 we regress the same three qualitative assessments used in Figure 4 on the same set of demographic controls in Table 2. The results are striking: the only mecro-economic pattern in Table 3 is found in different income groups' qualitative evaluation of the employment situation.[28] This shows that quantitative questions are able to capture additional meaningful variation that is difficult to observe in qualitative evaluations.

The only variables that consistently correlate with all three qualitative economic evaluations examined in Table 3 are partisan indicators. The next section uses both quantitative and qualitative questions to understand the sources of this partisan bias.

Thus, all three sets of patterns deduced above are consistent with the data. While this implies that our question is well-designed under the maintained hypotheses, it, by no means, can prove that it is well-designed. Our maintained hypothesis could be wrong, or there may be other implications contradicted by the data.

Finally, we note that including benchmarks has some potential drawbacks. The goal of benchmarks is to reduce the difference between respondents' perceptions of the economic conditions measured by the quantity of interest and their reported perceptions of the quantity—that is, to reduce measurement error. However, benchmarks may exacerbate, rather than reduce, measurement error. Some survey respondents might use information contained in the benchmark to form, rather than scale, their perceptions. This could occur if respondents know the current unemployment rate, but not the scale on which the unemployment rate is measured. Then providing the scale would cause respondents to update on the state of the economy, and perhaps alter responses to subsequent questions (Blinder and Krueger, 2004).

Benchmarks also could affect a respondent's ability or desire to express their true perception of the quantity. For example, our benchmarks may inhibit some respondents who know the scale on which the unemployment rate is measured from expressing their perception that

[28] Conducting the analyses in Table 3 using an ordered probit produces qualitatively similar results.

the unemployment rate is above its post-1948 peak.

While our benchmarks certainly are not perfect, we do not find much evidence that they generate the forms of measurement error discussed in the previous paragraph. While many forms of measurement error would attenuate the relationship between the reported unemployment rate and the qualitative economic evaluations, statistical tests contained in the appendix show the relationship is slightly stronger when using the benchmarked responses. We also don't find many cases of respondents survey satisficing by responding with the historical minimum, maximum, or average when they receive the benchmark. Between 2008 and 2009, the median reported unemployment rate in the benchmarked samples increased from 6.5% to 10.2%, which almost perfectly matched the actual change. Moreover, about 45 percent of responses in 2009 were above the historical maximum, as compared to 15 percent in 2008, suggesting that many respondents were willing to report perceptions above the historical maximum. Finally, providing respondents with benchmarks did not change responses to questions that followed in the survey.

5 Understanding Partisan Bias

Quantitative questions can be used to better understand variation in responses to qualitative questions. Qualitative economic questions often require respondents to make judgments about the quality of economic conditions, for example, whether jobs are "easy" or "hard" to find. Quantitative assessments do not involve such judgment. Thus, comparing quantitative and qualitative assessments can help determine the extent to which variation in responses to qualitative questions results from respondents using different criteria to judge economic conditions. In particular, we examine quantitative and qualitative assessments of unemployment to understand whether Democrats and Republicans judge economic conditions differently. We find preliminary evidence that partisanship either affects the reporting of both perceptions and evaluations, or affects economic judgments in a particularly odd

way: opponent partisans would have to be more lenient on the incumbent to rationalize the observed patterns.

Consistent with previous results, Table 4 shows that supporters of the incumbent party (Republicans in 2008), report more positive assessments of employment in both quantitative and qualitative questions (Wlezien, Franklin and Twiggs, 1997; Anderson, Mendes and Tverdova, 2004; Evans and Andersen, 2006; Evans and Pickup, 2010). However, Table 4 indicates that the qualitative reports are more related to partisan identification than the quantitative reports. In particular, nearly an identical number of respondents report that the unemployment rate is below 5.6% as report a positive evaluation of the employment situation. Yet, five times as many Republicans as Democrats report a positive qualitative evaluation of the employment situation (28.1% vs. 5.8%) compared with two and a half times as many Republicans as Democrats reporting an unemployment rate under 5.6% (24.7% vs. 10.2%).

The literature has identified three potential sources of partisan differences in responses to qualitative economic questions. First, perceptions of the economy may relate with partisan affiliations. This could occur either because partisanship directly affects economic perceptions (Gerber and Huber, 2010), or because partisanship is related to unmeasured determinants of economic perceptions, like personal experience with economy (Ansolabehere, Meredith and Snowberg, 2011*a*). Second, partisan affiliation may affect respondents' reports conditional on perceptions. Specifically, "partisan cheerleading" may cause supporters of the incumbent political party to report economic assessments that are more favorable than their actual economic perceptions. Third, and finally, partisanship may affect the criterion used to judge the economy. For example, a Democrat may judge that a 2% growth rate is "acceptable" when a Democrat is in power, but "unacceptable" when a Republican is in power. It is difficult to separate these three sources of partisan difference in cross-sectional data, as all three have the same effect: supporters of the incumbent report more positive evaluations of economic performance than opponents.[29]

[29]Previous research uses experimental or quasi-experimental variation in survey design to isolate survey effects (Wilcox and Wlezien, 1993; Palmer and Duch, 2001; Sturgis, Choo and Smith, 2009), or eschews

Preliminary evidence about the source of partisan bias in quantitative questions comes from comparing the correlates of reported unemployment rates on the 2008 and 2009 CCES. Over this time period Barack Obama (a Democrat) replaced George W. Bush (a Republican) in the White House. In contrast to 2008, Table A.1 in the Appendix shows that, in 2009, Republicans reported slightly higher rates of unemployment than Democrats. Moreover, other patterns of response remained largely the same as in 2008.[30] This suggests that differential economic experiences cannot be the only explanation for the partisan differences we observe in reported unemployment rates in Table 4. However, because quantitative assessments do not involve judgment, the partisan differences in reported unemployment rates observed in Section 4 must result either from differences in perceptions or in reporting.

Moreover, quantitative assessments can also be used to indirectly test for partisan differences in criteria used to judge the employment situation. To see this, consider a model where respondents randomly draw perceptions of the unemployment rate from a distribution that may vary by political party, and judge that perception according to whether it is higher or lower than a threshold. Respondents who perceive that the unemployment rate is below their evaluative threshold report a positive evaluation of unemployment, otherwise they report a negative evaluation. If supporters of the incumbent party apply a less stringent threshold when evaluating the economy, then supporters who report a positive evaluation will have a higher perception of the unemployment rate than opponents who report a positive evaluation. To put this another way: among those reporting a positive qualitative economic evaluation, the highest reported unemployment rates should come from members of the incumbent party. Likewise, among those reporting a negative qualitative economic evaluation, the lowest reported unemployment rates should come from members of the opposition party.

We do not find evidence consistent with criteria used to judge the employment situation being affected by partisanship. In contrast, Table 5 shows that in the 2008 CCES,

survey data altogether and considers consumption data (Gerber and Huber, 2009). Unfortunately, these techniques do not separate whether partisanship affects perceptions or judgments of economic conditions.

[30] As shown in Table A.1, differences in assessments between age groups were attenuated in 2009, while differences in assessments between race / ethnicity and education groups were enhanced.

Democrats generally report higher unemployment rates than Republicans, conditional on their qualitative assessments of unemployment.[31] For example, the interquartile range of reported unemployment rates among Republicans with a neutral qualitative evaluation is 5.8% to 7.0%. In comparison, the interquartile range of Independents and Democrats with a neutral qualitative evaluation is 6.0% to 7.5% and 6.0% to 8.5%, respectively. Thus, if partisans are using different criteria to judge the employment situation, it would have to be the case that Republicans are using a stricter criteria than Democrats. As this is counter to theory, we conclude that partisan differences mainly enter in reporting of economic assessments (whether elicited using qualitative or quantitative questions). Moreover, as the distribution of unemployment reports by party are less skewed than qualitative unemployment assessments, we conclude that these differences are less pronounced in quantitative questions.

6 Conclusion

Many theories in political science, such as theories of economic voting, are fundamentally rooted in numbers. In particular, the focus in the economic voting literature is on how vote shares change with changes in economic quantities such as GDP, inflation, or the unemployment rate. While survey questions that ask about numbers would form a tighter link between theory and survey data, numerous concerns have limited their use.

We have shown that survey respondents can handle quantitative questions, especially about familiar quantities such as the price of gas. Moreover, respondents' accuracy is affected by little else other than the regularity with which they are exposed to information about the price of gas. This finding stands in contrast to recent public opinion work demonstrating the inaccuracy of responses, particularly among certain types of individuals, to open-ended questions about quantities.[32]

[31] We cannot perform a similar analysis in 2009 because roughly 90% of the sample reports negative evaluations of the employment situation, see Footnote 24.

[32] For a few examples see Conover, Feldman and Knight (1986); Nadeau, Niemi and Levine (1993); Holbrook

Asking questions about more complex and unfamiliar quantities is a greater challenge. However, we are confident that these challenges can be overcome in many situations through careful question design. In particular, we show that providing information about the historical tendencies of a quantity can be quite useful in giving respondents a sense of the scale of the quantity in question, without obscuring meaningful variation in responses.

Quantities are important to voters' evaluations of many policies. In particular, budget and trade deficits, the cost of social programs, the number of people affected by a policy, and the number of war dead are all naturally expressed as numbers. The results here suggest that many of the barriers to using quantitative questions are surmountable, and provides some guidance on how they may be overcome.

That said, the value of quantitative questions goes beyond the ability to reduce measurement error or discover new, interesting patterns in the data. Many political economy theories are about specific quantities—GDP growth, levels of employment, tax policy, or changes in prices (inflation)—they are not about "feelings about the economy". Qualitative survey questions seem to conflate the underlying variables of interest—perceptions of economic quantities—with outcomes—evaluations of those perceptions and resulting political behavior. Thus, qualitative questions make, at best, an indirect statement about consumers' or voters' utilities. Those statements are not irrelevant, but it has long been known that direct comparisons of utilities are difficult. And, more importantly, they are not the primitives of theoretical models.

and Garand (1996); Kuklinski et al. (2000); Gilens (2001); Sigelman and Niemi (2001); Kaplowitz, Fisher and Broman (2003); Ansolabehere, Snowberg and Snyder (2005); Martinez, Wald and Craig (2008) and Herda (2010).

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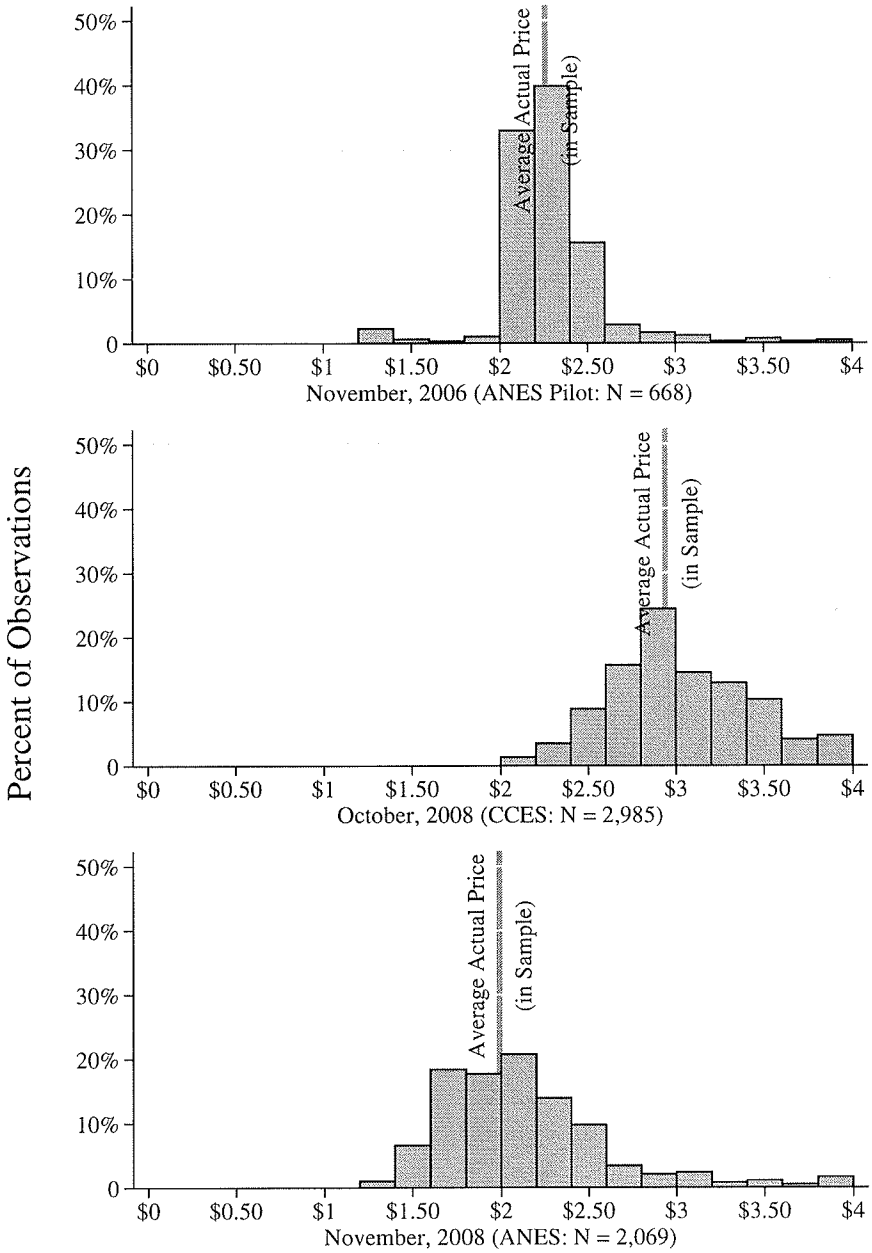
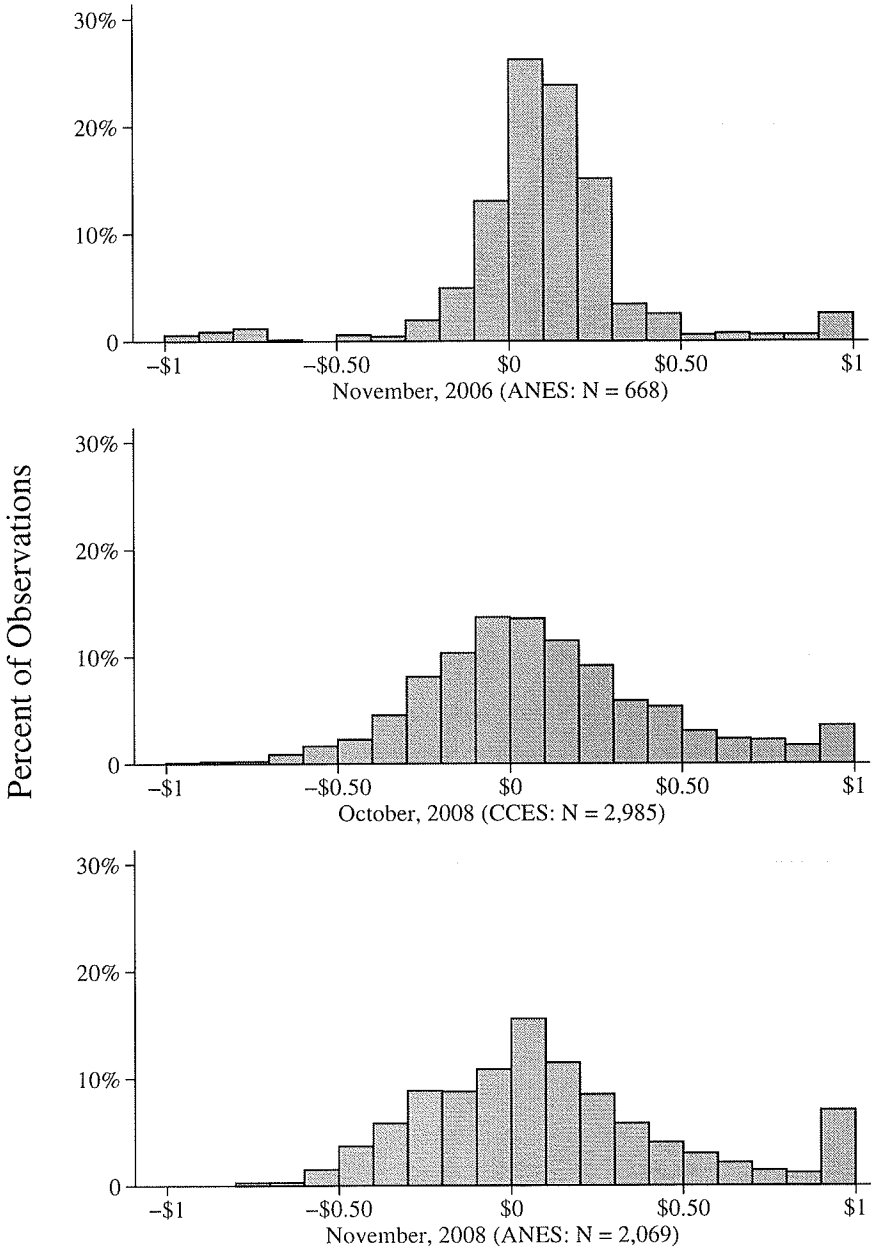
Figure 1: Distribution of Reports of Average State Gas Price



Notes: Top and bottom 1% of responses are set to the ninety-ninth and first percentile answer, respectively.

Figure 2: Distribution of Bias in Reports of Average State Gas Price



Notes: Errors are trimmed to be between -$1 and $1.

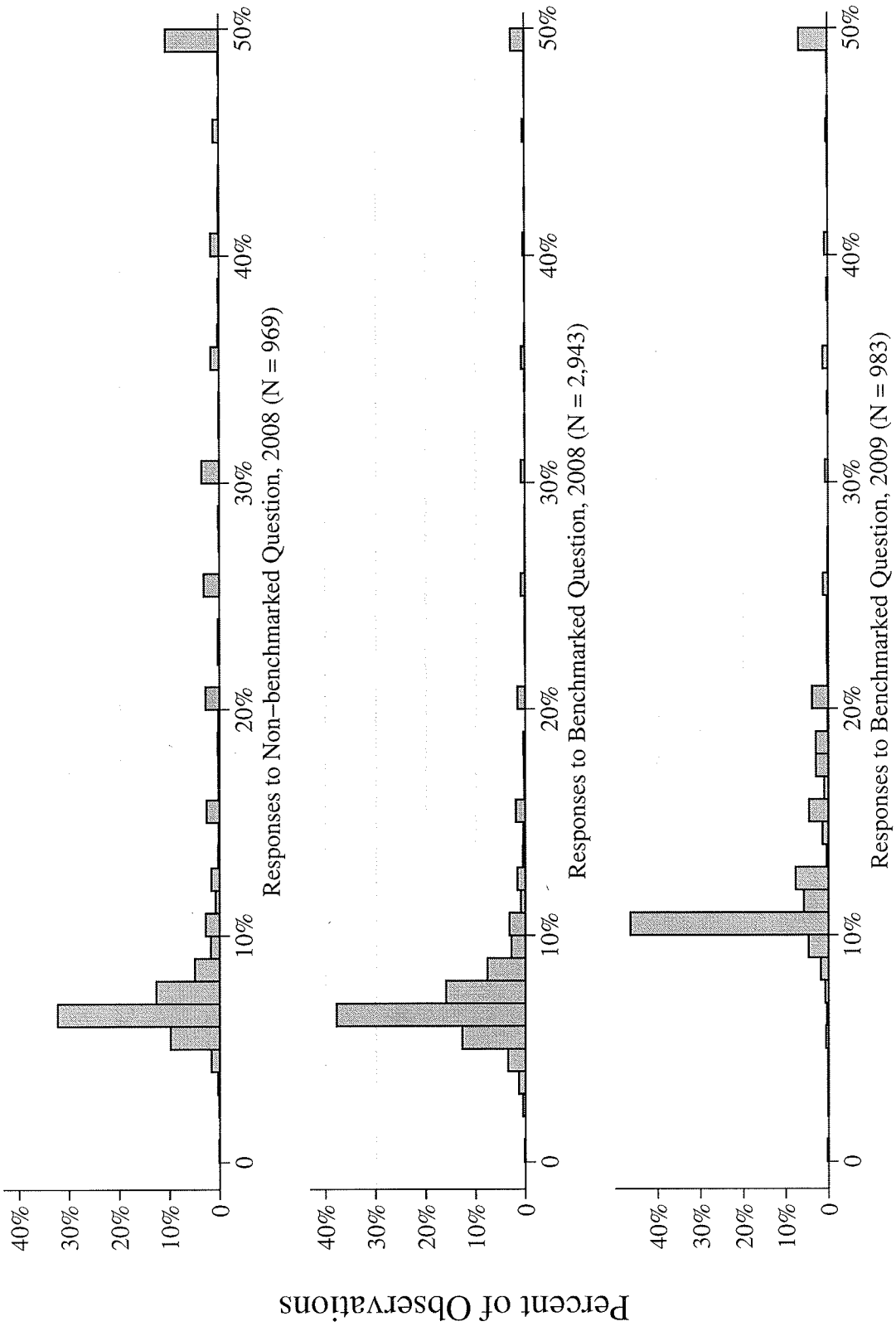
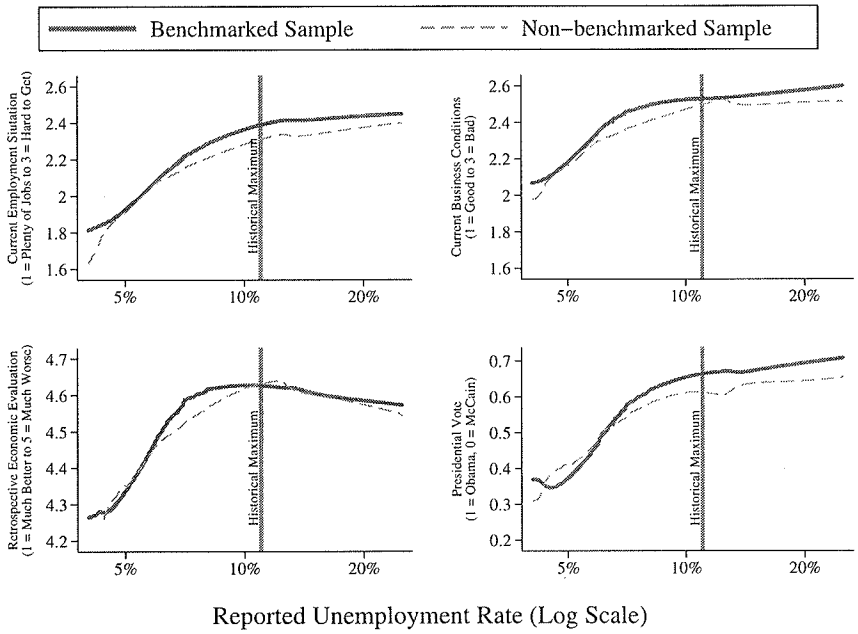


Figure 3: Distribution of Reports of the National Unemployment Rate

Figure 4: Relationship Between Qualitative and Quantitative Evaluations



Notes: Each figure is Loess smoothed using a bandwidth of 0.8 (Cleveland, Devlin and Grosse, 1988).

Table 1: Correlates of Reported Average Gas Price

	2006 ANES (N = 668)		2008 CCES (N = 2,985)		2008 ANES (N = 2,069)	
Dependent Variable	Bias	Accuracy	Bias	Accuracy	Bias	Accuracy
Mean Value	0.101	-0.193	0.102	-0.274	0.117	-0.292
Median Value	0.101	-0.131	0.058	-0.202	0.078	-0.215
Democrat	-0.020	-0.007	0.069***	-0.038***	0.016	0.006
	(0.027)	(0.021)	(0.017)	(0.012)	(0.025)	(0.018)
Independent	-0.015	0.012	0.069***	-0.039***	0.024	-0.013
	(0.028)	(0.022)	(0.017)	(0.012)	(0.025)	(0.018)
Age 18 - 44	0.070**	-0.065***	-0.045**	-0.002	-0.053***	0.032*
	(0.031)	(0.024)	(0.019)	(0.013)	(0.024)	(0.017)
Age 45 - 64	0.059**	-0.037*	-0.019	0.015	-0.041*	0.024
	(0.029)	(0.022)	(0.018)	(0.012)	(0.024)	(0.017)
Female	-0.011	-0.017	0.026**	-0.039***	0.010	-0.019
	(0.022)	(0.017)	(0.013)	(0.009)	(0.017)	(0.012)
Married	0.012	0.004	-0.057***	0.035***	-0.070***	0.037***
	(0.024)	(0.019)	(0.014)	(0.010)	(0.018)	(0.013)
Black	-0.035	-0.110***	0.037	-0.035**	0.025	-0.035**
	(0.037)	(0.029)	(0.023)	(0.016)	(0.023)	(0.016)
Hispanic	0.122**	-0.190***	0.055**	-0.046***	0.037*	-0.067***
	(0.055)	(0.042)	(0.023)	(0.016)	(0.022)	(0.015)
Some College	0.004	0.019	0.077***	-0.017	-0.011	0.017
	(0.028)	(0.021)	(0.016)	(0.011)	(0.020)	(0.014)
Bachelor's Degree	-0.016	0.019	0.107***	-0.032***	0.029	-0.007
	(0.028)	(0.022)	(0.017)	(0.012)	(0.023)	(0.016)
Income Less Than $20,000	0.047	0.016	0.021	-0.026	0.041	-0.026
	(0.048)	(0.037)	(0.030)	(0.021)	(0.040)	(0.028)
Income Between $20,000 and $40,000	0.000	-0.021	-0.057**	0.024	-0.030	0.012
	(0.043)	(0.033)	(0.025)	(0.018)	(0.038)	(0.027)
Income Between $40,000 and $80,000	-0.016	-0.006	-0.069***	0.027*	-0.059	0.033
	(0.037)	(0.028)	(0.022)	(0.016)	(0.037)	(0.026)
Income Between $80,000 and $120,000	-0.032	-0.028	-0.060**	0.040**	-0.026	-0.004
	(0.041)	(0.031)	(0.025)	(0.017)	(0.039)	(0.028)
Unemployed	-0.069	-0.057	0.092***	-0.041**	0.074**	-0.025
	(0.061)	(0.047)	(0.026)	(0.018)	(0.032)	(0.022)
Days Driving Per Week	-0.018***	0.008*				
	(0.006)	(0.004)				
Notice Gas Prices Per Week	-0.009**	0.016***				
	(0.005)	(0.004)				
Constant	0.187***	-0.232***	0.070**	-0.244***	0.172***	-0.309***
	(0.061)	(0.047)	(0.032)	(0.023)	(0.046)	(0.033)

Notes: ***, **, * denote statistical significance at the 1%, 5% and 10% level respectively. Regressions also include minor and missing party, church attendance, union membership, and missing income indicators. The omitted categories are Republicans, age 65+, male, not married, white, 12 years or less of education, $120,000+ for income, employed, not in union, and do not attend church. All regression estimated using OLS.

Table 2: Correlates of Reported Unemployment Rate in 2008 CCES

Dependent Variable	Level (LAD)			Percentile (OLS)		
Sample	Bench-marked	Non-bench-marked	Difference	Bench-marked	Non-bench-marked	Difference
Democrat	0.55***	1.50***	-0.95*	12.09***	9.07***	3.02
	(0.07)	(0.53)	(0.54)	(1.23)	(1.57)	(1.99)
Independent	0.31***	0.50	-0.19	8.12***	3.74***	4.38**
	(0.06)	(0.38)	(0.39)	(1.00)	(1.75)	(2.01)
Age 18 - 24	0.69	6.50	-5.81	12.84***	15.95***	-3.11
	(0.49)	(6.03)	(6.04)	(2.62)	(4.25)	(4.99)
Age 25 - 44	0.49***	1.50*	-1.01**	10.44***	10.10***	0.34
	(0.08)	(0.23)	(0.78)	(1.61)	(2.37)	(2.86)
Age 45 - 64	0.20***	0.50	-0.30	4.95***	5.74***	-0.79
	(0.05)	(0.51)	(0.51)	(1.32)	(1.79)	(2.22)
Married Male	0.17**	1.00*	-0.83	0.01	1.62	-1.61
	(0.07)	(0.60)	(0.61)	(1.48)	(2.83)	(3.20)
Unmarried Female	0.66***	3.00***	-2.34**	11.45***	12.16***	-0.71
	(0.10)	(1.24)	(1.24)	(1.50)	(3.21)	(3.54)
Married Female	0.62***	3.00***	-2.38*	10.31***	17.13***	-6.83**
	(0.11)	(1.06)	(1.07)	(1.34)	(2.49)	(2.82)
Black	0.61***	2.00	-1.39	6.90***	6.79**	0.11
	(0.23)	(2.98)	(2.99)	(1.41)	(2.92)	(3.24)
Hispanic	0.10	1.50	-1.40	0.39	4.37*	-3.98
	(0.13)	(1.54)	(1.54)	(2.15)	(2.46)	(3.27)
Some College	-0.30***	-1.50*	1.20	-5.21***	-8.25***	3.04
	(0.07)	(0.89)	(0.89)	(1.14)	(2.62)	(2.86)
Bachelor's Degree	-0.38***	-2.00**	1.62*	-7.35***	-12.99***	5.64**
	(0.07)	(0.93)	(0.94)	(1.28)	(2.43)	(2.75)
Income Less Than $20,000	0.77***	13.50***	-12.73*	10.46***	19.95***	-9.50
	(0.21)	(5.79)	(5.80)	(2.39)	(5.80)	(6.27)
Income Between $20,000 and $40,000	0.41***	3.00*	-2.59	6.55***	14.45***	-7.90
	(0.10)	(1.62)	(1.62)	(1.69)	(3.51)	(3.90)
Income Between $40,000 and $80,000	0.05	0.50	-0.45	2.88*	6.17**	-3.29
	(0.06)	(0.58)	(1.05)	(1.53)	(2.53)	(2.96)
Income Between $80,000 and $120,000	0.02	0.50	-0.48	0.72	2.98	-2.25
	(0.06)	(0.58)	(0.56)	(1.71)	(2.78)	(3.27)
Unemployed	0.20	1.00	-0.80	3.22*	1.94	1.28
	(0.18)	(0.56)	(2.17)	(1.80)	(2.99)	(4.45)
State Unemployment	0.12***	0.00	0.12	2.31***	0.52***	1.78***
	(0.02)	(0.13)	(0.13)	(0.36)	(0.57)	(3.50)
Constant	5.05***	4.50***		16.82***	24.04***	
	(0.16)	(1.37)		(3.62)	(5.60)	

Notes: ***, **, * denote statistical significance at the 1%, 5% and 10% level respectively, with standard errors block bootstraped by state in LAD regressions and clustered by state in OLS regressions. benchmarked: N=2,943; non-benchmarked: N=969. Regressions also include minor and missing party, church attendance, union membership, and missing income indicators. The omitted categories are Republicans, age 65+, male, not married, white, 12 years or less of education, $120,000+ for income, employed, not in union, and do not attend church.

Table 3: Correlates of Qualitative Economic Assessments in 2008 Benchmarked CCES

Dependent Variable	Retrospective Economic Evaluation	Business Conditions	Employment Situation
Democrat	0.53***	0.43***	0.40***
	(0.03)	(0.02)	(0.03)
Independent	0.31***	0.19***	0.22***
	(0.04)	(0.03)	(0.03)
Age 18 - 24	-0.09	-0.20***	-0.02
	(0.06)	(0.06)	(0.06)
Age 25 - 44	-0.04	0.02	0.03
	(0.04)	(0.04)	(0.04)
Age 45 - 64	-0.00	0.07*	0.07**
	(0.03)	(0.04)	(0.03)
Married Male	-0.09***	0.01	0.01
	(0.03)	(0.04)	(0.04)
Unmarried Female	0.00	0.06*	0.02
	(0.04)	(0.03)	(0.05)
Married Female	-0.11***	-0.01	-0.01
	(0.03)	(0.04)	(0.04)
Black	-0.02	-0.05	0.04
	(0.03)	(0.04)	(0.04)
Hispanic	-0.11**	-0.07**	-0.02
	(0.05)	(0.03)	(0.06)
Some College	0.03	0.01	-0.00
	(0.03)	(0.03)	(0.04)
Bachelor's Degree	0.03	0.00	-0.02
	(0.03)	(0.03)	(0.03)
Income Less	-0.09	0.02	0.30***
	(0.06)	(0.06)	(0.08)
Income Between $20,000 and $40,000	-0.10**	0.02	0.24***
	(0.05)	(0.04)	(0.04)
Income Between $40,000 and $80,000	-0.03	-0.05	0.11***
	(0.04)	(0.04)	(0.04)
Income Between $80,000 and $120,000	-0.04	-0.09**	0.03
	(0.04)	(0.04)	(0.04)
Unemployed	0.03	0.15***	0.18***
	(0.05)	(0.05)	(0.06)
State Unemployment	0.01	0.09***	0.09***
	(0.01)	(0.01)	(0.01)
Constant	4.34***	1.67***	1.27***
	(0.08)	(0.11)	(0.12)

Notes: ***, **, * denote statistical significance at the 1%, 5% and 10% level respectively with robust standard errors clustered at the state level. N=2,943. Regressions also include minor and missing party, church attendance, union membership, and missing income indicators. The omitted categories are Republicans, age 65+, male, not married, white, 12 years or less of education, $120,000+ for income, employed, not in union, and do not attend church.

Table 4: Unemployment Assessments by Party in 2008 Benchmarked CCES

	Partisan Identification		
	Republican	Independent	Democratic
Qualitative Unemployment Evaluation:			
Positive (N= 451)	28.1%	16.4	5.8
Neutral (N= 1,436)	52.1%	48.3	51.0
Negative (N= 960)	19.8%	35.3	43.2
Unemployment Rate:			
Less than 5.6 (N= 445)	24.7%	13.9	10.2
Between 5.6 and 7.0 (N= 1,497)	53.5%	55.6	49.3
Greater than 7.0 (N= 905)	21.8%	30.5	40.6

Notes: Numbers are percent of the column total.

Table 5: Conditional Distribution of Reported Unemployment Rates in 2008 Benchmarked CCES

	25th Percentile	50th Percentile	75th Percentile
Qualitative Unemployment Evaluation:			
Positive:			
Republican	5.1%	6.0	6.3
Independent	5.6%	6.1	7.0
Democratic	6.0%	6.5	8.0
Neutral:			
Republican	5.8%	6.0	7.0
Independent	6.0%	6.4	7.5
Democratic	6.0%	7.0	8.5
Negative:			
Republican	6.0%	7.0	8.9
Independent	6.1%	7.0	10.0
Democratic	6.1%	7.0	10.0

Appendix Not Intended for Publication

Between October 2008 and October 2009 the national unemployment rate increased from 6.5% to 10.2%. Additionally, Barack Obama (a Democrat) replaced George W. Bush (a Republican) in the White House. Did the types of individuals reporting relatively higher unemployment rates change from 2008 to 2009 as the economy worsened? Did partisan perceptions of the unemployment rate change with the party of the President?

Table A.1 shows a shift in how partisan identification relates to reported unemployment between 2008 and 2009. Whereas the median Democrat reported unemployment rates that were 0.55 percentage points (s.e. = 0.07) higher than the median Republican in 2008, the median Democrat reported unemployment rates that were 0.06 percentage points (s.e. = 0.15) lower than the median Republican in 2009. This finding is consistent with previous work showing that partisan attachments affect economic evaluations (Wlezien, Franklin and Twiggs, 1997; Anderson, Mendes and Tverdova, 2004; Evans and Andersen, 2006; Evans and Pickup, 2010).

Table A.1 also shows a number of differences in how demographics relate to reported unemployment in the 2008 and 2009 benchmarked CCES sample. First, respondents who did not attend college were relatively more negative about unemployment than those who attended college. In 2008, the median reported unemployment rate by respondents who had never attended college was 0.35 percentage points (s.e. = 0.07) higher than the median reported unemployment rate for respondents with a Bachelor's degree. In 2009, this gap increased to 1.10 percentage points (s.e. = 0.43). The point estimates in this table further show that Black and Hispanic respondents also became relatively more pessimistic about employment between 2008 and 2009, although these differences are not statistically significant at conventional levels. Unlike 2008, when younger respondents reported higher unemployment rates, we observe no differences across age groups in reported unemployment in 2009. Residents of states with more unemployment continued to report statistically significant higher unemployment rates in 2009, although the relationship is somewhat weaker

than in 2008.

We also compare the correlates of reported unemployment rates on the CCES with the correlates of reported unemployment rates on the ANES. As noted in the text, the 2006 and 2008 ANES also contained questions about the unemployment rate. The question on both surveys did not contain a benchmark, and the 2006 question differed slightly, as it asked respondents their perceptions of the unemployment rate in their state of residence. The results reported in Table A.2 show very similar patterns to those observed in the CCES. Not surprisingly, the point estimate on the effect of the state unemployment rate is larger in 2006 when the question is about state unemployment. The robustness of the results across time and survey suggest that the differences across groups in perceptions of unemployment are likely to hold in a wide variety of contexts.

The final table in the appendix provides statistical tests of the patterns observed in Figure 4. The patterns in Table A.3 show that the same general relationship holds between reported unemployment and qualitative assessments within the boundaries of the historical benchmarks in both the benchmarked and non-benchmarked sample. The first column shows that reported unemployment rates below 10.8% significantly relate to qualitative evaluations of the employment situation in the benchmarked sample, with a one percentage point increase in the reported unemployment rate associating with a 0.104 (s.e. = 0.010) units more negative qualitative evaluation of the employment situation. Similarly, the second column shows that a one percentage point increase in reported unemployment rates below 10.8% associates with a 0.071 (s.e. = 0.023) unit more negative qualitative evaluation of the employment situation in the non-benchmarked sample. We cannot reject the null that the difference between these two coefficients in the benchmarked and non-benchmarked sample, 0.033 (s.e. = 0.025), is statistically indistinguishable from zero at conventional levels. Similar patterns are found when we change our dependent variable to be retrospective economic evaluations, business conditions, or reporting a vote for Obama. These results are consistent with our hypothesis that our benchmark should not substantively change the relationship

between reported unemployment and measures we expect to associate with reported unemployment.

The patterns in Table A.3 also show that there is no statistical relationship between the reported unemployment above the historical maximum and qualitative evaluations in the benchmarked sample. These results are consistent with the respondents who report unemployment rates above the upper bound of the benchmark not attempting to answer the question.

Table A.1: Correlates of Reported Unemployment Rate in Benchmarked 2008 and 2009 CCES

Dependent Variable	Level (LAD)			Percentile (OLS)		
	2008	2009	Difference	2008	2009	Difference
Democrat	0.55***	-0.05	0.60***	12.09***	-3.44	15.53***
	(0.07)	(0.16)	(0.18)	(1.23)	(2.35)	(2.65)
Independent	0.31***	0.19	0.12	8.12***	3.57*	4.55*
	(0.06)	(0.19)	(0.20)	(1.00)	(2.12)	(2.34)
Age 18 - 24	0.69	-0.03	0.71	12.84***	0.48	12.36**
	(0.48)	(0.39)	(0.61)	(2.62)	(4.66)	(5.34)
Age 25 - 44	0.49***	-0.10	0.59**	10.44***	-2.05	12.49***
	(0.08)	(0.22)	(0.23)	(1.61)	(2.40)	(2.89)
Age 45 - 64	0.20***	-0.13	0.33	4.95***	-3.17	8.12**
	(0.05)	(0.23)	(0.24)	(1.32)	(2.90)	(3.19)
Married Male	0.17**	0.32	-0.15	0.01	6.56***	-6.55**
	(0.07)	(0.23)	(0.24)	(1.48)	(2.39)	(2.81)
Unmarried Female	0.66***	0.30	0.35	11.45***	6.17**	5.27
	(0.10)	(0.25)	(0.27)	(1.50)	(2.85)	(3.22)
Married Female	0.62***	0.58**	0.05	10.31***	10.27***	0.04
	(0.11)	(0.29)	(0.16)	(1.34)	(2.74)	(3.05)
Black	0.61**	0.76	-0.16	6.90***	11.83***	-4.93
	(0.23)	(0.55)	(0.59)	(1.41)	(2.93)	(3.25)
Hispanic	0.10	0.14	-0.04	0.39	1.60	-1.21
	(0.13)	(0.23)	(0.26)	(2.15)	(2.58)	(3.36)
Some College	-0.30***	-1.03**	0.73*	-5.21***	-11.73***	6.53***
	(0.07)	(0.43)	(0.44)	(1.14)	(2.21)	(2.44)
Bachelor's Degree	-0.38***	-1.16***	0.78*	-7.35***	-12.88***	5.53**
	(0.07)	(0.43)	(0.43)	(1.28)	(2.13)	(2.49)
Income Less Than $20,000	0.77***	3.21*	-2.44	10.46***	9.41***	1.05
	(0.21)	(1.81)	(1.82)	(2.39)	(3.47)	(4.21)
Income Between $20,000 and $40,000	0.41***	0.04	0.37	6.55***	3.47	3.08
	(0.10)	(0.39)	(0.40)	(1.69)	(3.64)	(4.01)
Income Between $40,000 and $80,000	0.05	0.08	-0.03	2.88*	0.14	2.74
	(0.06)	(0.16)	(0.17)	(1.53)	(2.75)	(3.15)
Income Between $80,000 and $120,000	0.02	0.02	0.01	0.72	1.02	-0.29
	(0.06)	(0.16)	(0.17)	(1.71)	(3.04)	(3.49)
Unemployed	0.20	0.17	0.03	3.22*	2.50	0.72
	(0.18)	(0.38)	(0.43)	(1.80)	(3.15)	(3.63)
State Unemployment	0.12***	0.07*	0.05	2.31***	1.29***	1.02*
	(0.02)	(0.04)	(0.04)	(0.36)	(0.41)	(0.54)
Constant	5.05***	10.36***		16.82***	37.07***	
	(0.16)	(0.60)		(3.62)	(5.84)	

Notes: ***, **, * denote statistical significance at the 1%, 5% and 10% level respectively with robust standard errors block bootstraped by state in LAD regressions and clustered by state in OLS regressions. 2008: N=2,943; 2009: N=983. Regressions also include minor and missing party, church attendance, union membership, and missing income indicators. The omitted categories are Republicans, age 65+, male, not married, white, 12 years or less of education, $120,000+ for income, employed, not in union, and do not attend church.

Table A.2: Correlates of Reported Unemployment Rate in ANES

Dependent Variable	Level (LAD)		Percentile (OLS)	
	2006	2008	2006	2008
Democrat	2.33**	3.16*	7.18**	5.90***
	(1.11)	(1.85)	(2.38)	(1.46)
Independent	0.84	3.55**	5.41***	6.33***
	(0.96)	(1.40)	(1.80)	(1.30)
Age 18 - 24	3.51	8.17**	6.88	7.64**
	(3.72)	(3.71)	(5.97)	(2.49)
Age 25 - 44	3.17*	5.22***	7.47**	6.73***
	(1.66)	(1.81)	(3.11)	(2.26)
Age 45 - 64	1.00	2.45	2.68	2.15
	(1.38)	(1.63)	(2.84)	(2.16)
Married Male	-0.14	3.16**	-3.84	-0.41
	(1.31)	(1.52)	(3.02)	(2.10)
Unmarried Female	5.61***	9.24***	11.30***	9.81***
	(2.14)	(1.58)	(2.35)	(1.22)
Married Female	2.03	7.79***	9.17***	10.06***
	(1.84)	(2.12)	(2.83)	(1.77)
Black	15.88***	20.81***	18.24***	15.26***
	(4.98)	(2.99)	(3.20)	(1.37)
Hispanic	1.70	16.44***	3.42	13.64***
	(8.18)	(3.93)	(5.55)	(2.22)
Some College	-3.14	-10.54***	-3.68	-8.92***
	(2.71)	(2.22)	(3.17)	(1.46)
Bachelor's Degree	-4.68**	-15.92***	-14.16***	-19.23***
	(2.26)	(2.11)	(2.79)	(1.34)
Income Less Than $20,000	5.44	9.00***	7.77*	9.83***
	(4.38)	(2.20)	(4.40)	(3.00)
Income Between $20,000 and $40,000	4.82	0.69	9.43***	3.08
	(3.08)	(2.07)	(3.63)	(2.70)
Income Between $40,000 and $80,000	0.38	-0.60	1.42	2.34
	(1.60)	(1.53)	(3.22)	(2.24)
Income Between $80,000 and $120,000	-0.26	-2.34	0.65	-2.32
	(1.16)	(1.72)	(2.79)	(2.32)
Unemployed	11.59*	2.96	8.05	2.76
	(6.27)	(2.81)	(5.15)	(1.74)
State Unemployment	1.22*	0.05	3.57***	0.89
	(0.69)	(0.54)	(1.05)	(0.53)
Constant	2.15	12.41***	21.68***	26.19***
	(3.82)	(4.55)	(6.11)	(4.24)

Notes: ***, **, * denote statistical significance at the 1%, 5% and 10% level respectively with robust standard errors block bootstraped by state in LAD regressions and clustered by state in OLS regressions. 2006: N=656; 2009: N=1,925. Regressions also include minor and missing party, church attendance, union membership, and missing income indicators. The omitted categories are Republicans, age 65+, male, not married, white, 12 years or less of education, $120,000+ for income, employed, not in union, and do not attend church. Standard errors block bootstraped by state in LAD regressions and clustered by state in OLS regressions.

Table A.3: Comparing Quantitative and Qualitative Economic Assessments in 2008 CCES

Dependent Variable	Employment Situation		Retrospective Evaluation		Business Conditions		Obama Vote Choice	
Sample	Bench-marked	Non-bench-marked	Bench-marked	Non-bench-marked	Bench-marked	Non-bench-marked	Bench-marked	Non-bench-marked
Centered Unemployment X	0.104***	0.072***	0.076***	0.068***	0.078***	0.048***	0.058***	0.068***
(1 - Above 10.8%)	(0.010)	(0.023)	(0.010)	(0.022)	(0.009)	(0.021)	(0.008)	(0.017)
Difference in Slope		0.032		0.008		0.030		-0.010
Below 10.8%		(0.025)		(0.025)		(0.023)		(0.019)
Centered Unemployment X	0.002	0.002	0.001	-0.000	0.002	0.002*	0.001	0.004***
Above 10.8%	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Above 10.8%	-0.184***	-0.069	-0.281***	-0.202*	-0.198***	-0.064	-0.084	-0.276***
	(0.061)	(0.122)	(0.061)	(0.112)	(0.056)	(0.106)	(0.052)	(0.090)
Constant	2.59***	2.40***	4.85***	4.78***	2.72***	2.52***	0.769***	0.817***
	(0.044)	(0.107)	(0.044)	(0.101)	(0.042)	(0.095)	(0.037)	(0.076)

Notes: *** ** * denote statistical significance at the 1%, 5% and 10% level respectively. All regressions estimated using OLS with robust standard errors (N = 2,943 benchmarked, 969 non-benchmarked). To make the coefficient on the indicator for a response above 10.8% interpretable, we center the percentile by subtracting a constant, as standard in the regression discontinuity literature (Imbens and Lemieux, 2008). The centered percentile measure is the difference between the percentile of reported unemployment and the percentile that a reported unemployment of 10.8% takes in the sample. Thus, the constant captures the expected value for those reporting an unemployment rate of 10.8%, while the indicator captures the discontinuous change in the expected value for those reporting an unemployment rate above 10.8%.

Exhibit F

Exhibit F

REGIONAL DIFFERENCES IN RACIAL POLARIZATION IN THE 2012 PRESIDENTIAL ELECTION: IMPLICATIONS FOR THE CONSTITUTIONALITY OF SECTION 5 OF THE VOTING RIGHTS ACT

Stephen Ansolabehere, Nathaniel Persily & Charles Stewart III[*]

I. INTRODUCTION

Three years ago, when the Supreme Court last considered the constitutionality of the coverage formula of section 5 of the Voting Rights Act of 1965[1] (VRA), we submitted an amicus brief on behalf of neither party analyzing the relevance to the case of voting patterns in the 2008 election.[2] In particular, the brief, and a subsequent *Harvard Law Review* article that expanded upon it,[3] highlighted relative rates of racially polarized voting in the covered and noncovered jurisdictions to demonstrate where racial polarization had increased over time. Although some states had seen increases and others had seen decreases in the gap in candidate preferences between racial groups, the brief and article concluded that, contrary to much conventional wisdom, racial polarization had actually increased in the 2008 election, especially in the areas covered by section 5 of the VRA.

We find ourselves in much the same position now as we did three years ago. We also find ourselves coming to the same conclusions, which have become, if anything, more strongly supported by recent

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[1] 42 U.S.C. § 1973c (2006).

[2] Brief for Nathaniel Persily et al. as Amici Curiae on Behalf of Neither Party, Nw. Austin Mun. Util. Dist. No. One v. Holder (*NAMUDNO*), 129 S. Ct. 2504 (2009) (No. 08-322), *available at* http://www.law.columbia.edu/null/download?&exclusive=filemgr.download&file_id=151457. The brief was mentioned by Justice Kennedy at the oral argument, *see* Transcript of Oral Argument at 55–56, *NAMUDNO*, 129 S. Ct. 2504 (No. 08-322), *available at* http://www.supremecourt.gov/oral_arguments/argument_transcripts/08-322.pdf, and cited by numerous commentators at the time, *see, e.g.*, Robert Barnes, *High Court to Weigh Relevance of Voting Law in Obama Era*, WASH. POST, Apr. 1, 2009, at A1; Adam Liptak, *Review of Voting Rights Act Presents a Test of History v. Progress*, N.Y. TIMES, Apr. 28, 2009, at A16; Jeffrey Toobin, *Voter, Beware*, NEW YORKER, Mar. 2, 2009, at 19; Linda Greenhouse, *There's Always Another Day*, SLATE (June 22, 2009, 1:39 PM), http://www.slate.com/id/2220927/entry/2221036.

[3] Stephen Ansolabehere, Nathaniel Persily & Charles Stewart III, *Race, Region, and Vote Choice in the 2008 Election: Implications for the Future of the Voting Rights Act*, 123 HARV. L. REV. 1385 (2010).

data. Voting in the covered jurisdictions has become even more polarized over the last four years, as the gap between whites and racial minorities has continued to grow. This is due both to a decline among whites and an increase among minorities in supporting President Obama's reelection. This gap is not the result of mere partisanship, for even when controlling for partisan identification, race is a statistically significant predictor of vote choice, especially in the covered jurisdictions.

Even more now than four years ago, both sides in the VRA debate look to the most recent election to support their case. Critics of the VRA point to the reelection of the nation's first African American president, amidst record rates of minority voter turnout, as evidence of how "times have changed" since 1965. The "strong medicine"[4] of the VRA is no longer needed in the South, they argue, because the historic barriers to minority participation and office holding have largely vanished. For supporters of the VRA, the history since 1965 and the 1982 reauthorization demonstrate the continuing danger to minority voting rights in the covered jurisdictions. They point also to this past election as confirming Congress's suspicions in the reauthorization process as new obstacles to voting, such as photo identification laws and restrictions on early voting, were more prevalent in the covered states. In the run up to the 2012 election, section 5 proved it had bite, as photo ID and other laws were prevented from going into effect by the Department of Justice (DOJ) or the district court in Texas, South Carolina, and Florida, and Texas's congressional redistricting plan was found to be intentionally discriminatory.

These contrasting views of the relevance of the 2012 election may very well provide the media frame for the debate over section 5 in the current challenge to the VRA in *Shelby County v. Holder*.[5] Of course, the contending narratives of "look how far we've come" versus "see how much voting discrimination persists" are usually not the stuff of constitutional arguments. Moreover, the results of a highly salient and well-funded presidential election may seem beside the point for the constitutionality of a law that has its greatest effect in the context of local, below-the-radar election law changes.

All involved in the debate over the VRA must admit, however, that we do not know exactly what the world will look like if section 5 is struck down. Of course, the South would not revert back to Jim Crow days: politics has evolved beyond the days of threatened lynchings for

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[4] *Voting Rights: Hearings on H.R. 6400 and Other Proposals to Enforce the 15th Amendment to the Constitution of the United States Before Subcomm. No. 5 of the H. Comm. on the Judiciary*, 89th Cong. 110 (1965) (statement of Rep. Frank Chelf, Member, H. Comm. on the Judiciary).

[5] 133 S. Ct. 594 (2012).

the exercise of the franchise. But the many examples in the legislative record of voting rights violations prevented by the VRA hint at what might happen if the covered jurisdictions were otherwise unconstrained. Even if Jim Crow will not return, the familiar regional pattern of discrimination might, as new stratagems replace old ones with minority voters becoming collateral damage in increasingly vicious partisan fights.

The litigants in *Shelby County* disagree over the applicable constitutional test and the necessary evidentiary showing for upholding the VRA. In particular, the challengers assert that Congress needed to distinguish the covered from the noncovered jurisdictions, in order to demonstrate that the coverage formula captures the areas of the country (and only those areas) that pose the greatest threat to minority voting rights. From their perspective, the coverage formula can only be congruent and proportional[6] (and therefore constitutional) if it is precisely tailored to capture only "guilty" jurisdictions and no "innocent" ones.

Although defenders of the VRA point to higher rates of successful section 2 VRA cases as one example of where the covered states have distinguished themselves as voting rights violators, they also maintain that Congress need only justify continued coverage by finding persistent dangers to voting rights in covered areas alone. The coverage formula, from its inception, has always been over and underinclusive of the jurisdictions of concern. Overinclusivity is addressed by the bailout provision, which allows "good" jurisdictions to escape coverage when they can demonstrate a clean voting rights record. So long as the coverage-formula-plus-bailout regime represents a rational attempt to address the problem of minority voting rights violations, defenders argue, the law is constitutional.

The challengers' argument against the coverage formula would put Congress in an awkward position whenever justifying a geographically specific civil rights law. If the covered jurisdictions remain completely unchanged in their disrespect for minority voting rights, then the VRA is not working as promised. On the other hand, successful deterrence of voting rights violations in the covered states becomes evidence of the statute's unconstitutionality if those jurisdictions become less distinct. In the oral argument in *Northwest Austin Municipal Utility District No. One v. Holder*[7] (*NAMUDNO*), Chief Justice John Roberts described this problem as "the Elephant Whistle problem." To summarize the allegory: A guy with a whistle around his neck walks into a bar. Another guy asks him, "Why are you wearing a whistle around

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[6] City of Boerne v. Flores, 521 U.S. 507, 520 (1997).

[7] 129 S. Ct. 2504 (2009).

your neck?" "It's to keep away elephants," the first responds. "How do you know it's working?" the second asks. "Do you see any elephants around here?"[8]

If the Court takes the elephant whistle problem seriously, the challenge for defenders of the VRA is to find a metric that can hint at the danger of the VRA's removal while simultaneously not suggesting it either has been ineffective or has outlived its usefulness. To some extent, the number of preclearance denials and DOJ requests for more information provide such metrics by pointing at the types of laws that would have gone into effect but for the existence of the VRA. But even those data are incomplete because they cannot pick up the VRA's deterrent effect — that is, the laws that were never proposed or passed because politicians knew they would not be allowed to go into effect. One should expect the number of laws denied preclearance to be small as compared to the number of laws that are never passed because of the VRA's deterrent effect.

II. RACIAL POLARIZATION AS AN INDICATOR OF AREAS OF MINORITY VOTING RIGHTS CONCERN

The degree of racial polarization in an electorate can be the kind of exogenous indicator of potential threats to minority voting rights that is not directly affected by the presence of VRA coverage. Although one might expect the VRA to have some indirect effect over time on metrics of racial harmony[9] — and indeed, the covered areas are very different along those metrics than they were in 1965 — the existence of adverse political preferences between minorities and whites exists by itself as a kind of danger sign as to what might happen if the VRA were to vanish.

In particular, in states with high concentrations of minorities and a white majority unwilling to cross over to vote for minority-preferred candidates, we might expect several dangers to be present from a minority voting rights perspective. The first and most obvious is that by definition, areas of high racial polarization are ones where minorities will have less of a chance of electing politicians they prefer and that will be responsive to the minority community. Indeed, this is the

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[8] *See* Jeffrey Toobin, *No More Mr. Nice Guy: The Supreme Court's Stealth Hard-Liner*, NEW YORKER, May 25, 2009, at 42, 42 ("That's like the old elephant whistle," he said. "You know, 'I have this whistle to keep away the elephants.' You know, well, that's silly. 'Well, there are no elephants, so it must work.'").

[9] *See* Heather K. Gerken, *Keynote Address: What Election Law Has to Say to Constitutional Law*, 44 IND. L. REV. 7, 10–14 (2010) (supporting the creation of majority-minority districts as a tool for furthering integration); Michael S. Kang, *Race and Democratic Contestation*, 117 YALE L.J. 734, 778–84, 787 (2008) (arguing that the creation of majority-minority districts reduces racial polarization).

theory undergirding the redistricting jurisprudence for section 2 of the VRA: although minorities will lose in a majority-rule system, there is something inherently wrong with a system in which a large racial group is systematically outvoted and unrepresented by redistricting schemes that disadvantage them.

Second, when political preferences fall along racial lines, the natural inclinations of incumbents and ruling parties to entrench themselves have predictable racial effects. Under circumstances of severe racial polarization, efforts to gain political advantage translate into race-specific disadvantages. For example, a ruling party or coalition that seeks to hobble the competitive position of its adversary by making it more difficult for their constituencies to vote or campaign will inevitably discriminate against a racial group. In those circumstances, race-based discrimination becomes an efficient tool for incumbent protection or partisan advantage.

There are several responses to the claim that geographic patterns of racial polarization can be important evidence in support of the coverage formula. The first, articulated by Justice Thomas in his separate opinion in *NAMUDNO*, is that "racially polarized voting is not evidence of unconstitutional discrimination [and] is not state action."[10] Regardless of the fact that elections choose state actors and such private choices occur in a state-structured environment, individual voting, on this score, is private action, just like individual speech. Even if such private choices arise from racial animus, geographic patterns in how those choices are made are viewed as outside the realm of permissible evidence for justifying Congress's power to enforce voting rights by way of the VRA. According to this approach, only the existence of unconstitutional laws or regulations — or more properly, the relative predominance of such laws in covered areas — can justify geographically targeted voting rights laws.

Defenders of the VRA might also agree that state violations of voting rights are better evidence to support congressional efforts in this area. After all, a state that disenfranchises racial minorities, but in which there is high white crossover voting, would still be one deserving of special federal attention. If, from the beginning, one had to choose among possible metrics for determining coverage, unconstitutional laws, as opposed to voting behavior, would be the most appropriate basis for distinguishing between institutions. Of course, the original VRA combined the two, designating for coverage jurisdictions that both used a test or device and had low voter turnout. Neither individually nor in combination were those factors unconstitutional,

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[10] *NAMUDNO*, 129 S. Ct. at 2526 (Thomas, J., concurring in the judgment in part and dissenting in part) (citation omitted).

however. Rather, they were seen as indicators of likely unconstitutional action, and the record showed that they captured most of the jurisdictions of concern. Indeed, the coverage formula was reverse engineered to do so.

The evidence required to justify the constitutionality of the coverage formula today cannot be the same that justified the law when it was enacted. Otherwise, the law would have been unconstitutional soon after it was enacted, as minority political participation and office holding increased considerably and literacy tests vanished. Moreover, the statute cannot be constitutionally disadvantaged for its unique sunset period (which had previously been considered one of its saving graces). Most civil rights laws probably accomplish their goals or improve circumstances compared to their date of passage. They do not become immediately unconstitutional as a result. Congress's decision to require reauthorization should not then trigger a constitutional test for a reauthorized law that would be different than one originally passed without a sunset period.

Nevertheless, as it is the reauthorized version of the statute that is under review, evidence of the dangers of its removal could be valuable in assessing its continued constitutionality. There are strong and weak forms of the argument that racial polarization patterns in recent presidential elections support the constitutionality of section 5. The weak form merely dispels the notion that the election and reelection of an African American President should put section 5 to rest. The persistence of racial polarization in the covered areas — and in some cases, increased racial polarization — points to the complicated trends in voter behavior masked by President Obama's reelection.

The strong version of the argument is that the differential patterns of racial polarization demonstrate the constitutionality of section 5. As with any other piece of evidence concerning this reauthorization, previous reauthorizations, or even the original VRA, the patterns of relevant conduct (in this case, racially polarized voting) do not map perfectly onto the coverage designations. There are some noncovered areas with higher rates of racial polarization than some covered areas, and vice versa.

We can, however, demonstrate that racial polarization is higher, *on average*, in the covered areas than the noncovered areas. We can also demonstrate that the extent of racial polarization in presidential elections increased over the past decade. Even when we account for partisan identification, the differences in rates of racial polarization between the covered and noncovered areas remain statistically significant.

III. RACIAL POLARIZATION IN PRESIDENTIAL ELECTIONS, 1984–2012

A. Racial Polarization, 1984–2008

Racially polarized voting is a term of art in voting rights law. It refers not to racist voting but to a high correlation between vote choice and race. As Justice Brennan's opinion in *Thornburg v. Gingles*[11] explained:

> [T]he legal concept of racially polarized voting incorporates neither causation nor intent. It means simply that the race of voters correlates with the selection of a certain candidate . . .; that is, it refers to the situation where different races (or minority language groups) vote in blocs for different candidates.[12]

Because the secret ballot prevents us from knowing the candidate choice of each voter by race, we need to rely on aggregated or sampled data to make inferences about how groups voted. As before, we look at survey data and ecological regression analysis to surmise the likely split of the vote along racial lines. Surveys can come from exit polls or other sources, but the sample must be large enough to get representative numbers for each of the states. Ecological regression relies only on election results and census race statistics. For each county, we can identify its racial composition and the vote share received by each candidate. With each county existing as one data point on a graph that arrays minority percentage on the X-axis and vote share received by the Democratic candidate on the Y-axis, we can estimate the likely share of the white and minority vote received by each candidate. The slope of the regression line that fits the data can also tell us how much increased vote share the Democratic candidate can expect to receive for each percent increase in the minority population of a jurisdiction. (We use the Democratic candidate for simplicity's sake; we could use the Republican candidate and it would show the same dynamic, except inverse.) The graphs present different lines for the covered and non-covered counties. By comparing the slope and Y-intercept of those lines, we can assess the relative importance of race as a predictor of presidential vote choice for each class of jurisdictions.

Our previous article detailed the well-known racial and regional differences in presidential voting patterns according to statewide exit polls from 1984 to 2004.[13] Over this period, minority voters supported the Democratic candidate relatively consistently and regardless of the coverage status of a jurisdiction. African Americans, in either type of

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[11] 478 U.S. 30 (1986).
[12] *Id.* at 62 (opinion of Brennan, J.).
[13] See Table 1 and Figure A for a summary of our findings.

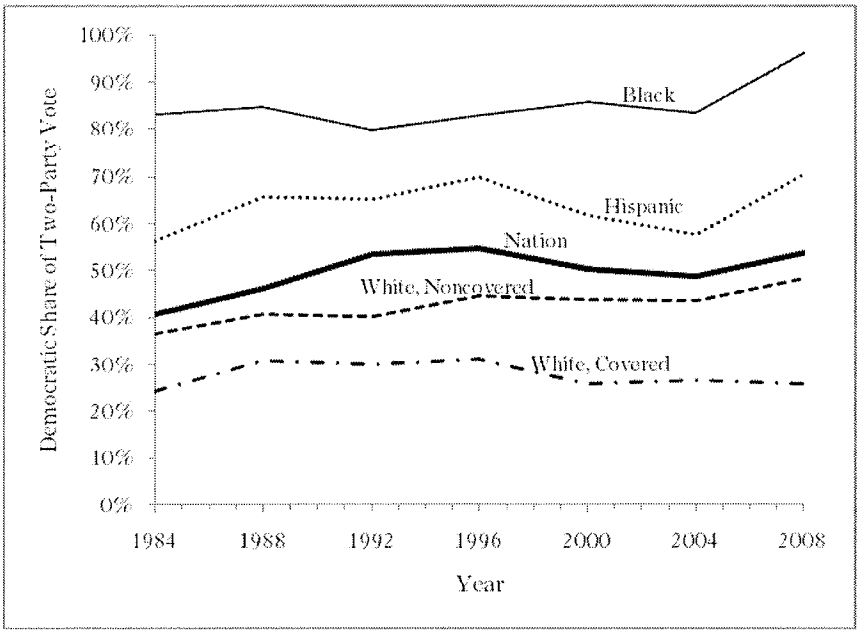
jurisdiction, supported the Democratic candidate at a rate of 84%. Latinos were less pronounced or consistent in their support, but 61% of Latinos in the covered and 64% in the noncovered (or partially covered) jurisdictions supported the Democratic candidate, on average. The divergence between the covered and noncovered states is most pronounced among whites. White support for the Democrat in the covered states during this period lagged support in the noncovered or partially covered states by fourteen percentage points — 42% to 28%. Racial polarization between whites and blacks — that is, the difference in Democratic support from whites as compared to blacks — was forty-two percentage points for the noncovered jurisdictions and fifty-six percentage points for the covered jurisdictions. The gap between Latinos and whites was smaller — twenty-two percentage points for the noncovered jurisdictions and thirty-three percentage points for the covered jurisdictions.

TABLE 1. THE RACIAL GAP IN VOTING FOR DEMOCRATIC NOMINEE, PRESIDENTIAL EXIT POLLS, 1984–2004[14]

Group	Covered	Noncovered + Partially Covered	Nation
White	28	42	39
Black	84	84	84
Latino	61	64	63
Whites			
Democrats	72	79	78
Republicans	4	9	8
Independents	28	42	40
Difference			
Black-white	56	42	45
Latino-white	33	22	24

[14] These data were gathered from national exit polls archived at INTER-UNIVERSITY CONSORTIUM FOR POLITICAL AND SOCIAL RESEARCH (ICPSR), https://www.icpsr.umich.edu/icpsrweb/ICPSR/access/series.jsp (last visited April 6, 2013). All calculations were performed using sample weights provided by the exit poll in the relevant file. In all ICPSR files, the weight variables are labeled WGT. The exit poll results are weighted to reflect the complexity of the sampling design and to take into account the different probabilities of selecting a precinct and of selecting a voter within each precinct. The weights are defined such that the exit poll results equal the final tabulated vote within geographic regions of the states or nation. Calculations were made for each state using the within-state weights provided by the exit polls. Next, aggregate calculations were made for VRA and non-VRA regions, weighting each state by the population of interest (i.e. Whites, Blacks, Hispanics, White Democrats, White Republicans, and White Independents) residing in that state.

FIGURE A. DEMOCRATIC CANDIDATE'S SHARE OF THE TWO-PARTY VOTE, NATIONAL EXIT POLLS, 1984–2008



B. Racial Polarization in 2008

As compared to the twenty-year trend that preceded it, racial polarization increased in the 2008 presidential election. In both the covered and noncovered states, Barack Obama received a large, above-average share of the minority vote, such that the white-black and white-Latino gap increased. However, in the covered states, his voteshare among whites dropped two points from the historical average (tying the figure in 2004, we should note). In contrast, in the noncovered states, he increased his white voteshare by six percentage points.

The 2008 election highlights how racial polarization — the difference between the minority voteshare and white voteshare received by the minority-preferred candidate — can increase either through a decline in the white voteshare received by the candidate or through an increase in the minority voteshare received (or both). In the noncovered states, Barack Obama increased his voteshare among whites and minorities. In the noncovered states his share of the white vote was below average for a Democrat, while his share of the minority vote was well above average, especially among African Americans.

Table 2 compares Obama's voteshare by race with that of John Kerry's losing effort four years earlier. The exit polls illustrate that the jump in white support he received was mainly due to increases in the noncovered areas — the big exception being Virginia where he received eight points more of the white voteshare than did Kerry. However, in several covered states, white support for Obama dropped dramatically from four years earlier. In Alabama, Mississippi, and Louisiana, for example, Obama received only 10%, 11%, and 14% of the white vote respectively, which was nine, three, and ten percentage points less than Kerry received from whites in that state.[15] Despite very favorable conditions for the "out-party" candidate in 2008, Obama did not improve on Kerry's average performance among whites in the covered states and dropped significantly in several of them.

TABLE 2. RACIAL GAP IN PRESIDENTIAL VOTING PREFERENCES, 2008 EXIT POLLS[16]

	Covered States		Noncovered States		Nation	
Group	2008 (%)	Change from 2004	2008 (%)	Change from 2004	2008 (%)	Change from 2004
White	26	0	48	4**	44	3**
Black	97	9**	96	9**	96	9**
Latino	67	16**	72	9**	70	11**
Whites						
Democrats	75	-7**	85	0	84	-1**
Republicans	4	1**	10	4**	9	3**
Independents	31	-3	50	-2	47	-2
Difference						
Black-white	71	9**	48	5**	52	6**
Latino-white	41	16**	24	5**	26	8**

[15] *See* Ansolabehere et al., *supra* note 3, at 1422–23. As noted, Obama improved significantly among whites in Virginia, and in heavily covered states such as North Carolina and New York, he performed much better than previously. The only other outlier worth noting is the drop of six points among whites in noncovered Arkansas, where Obama nevertheless still got 31% of the white vote.

[16] **p<0.01. Exit poll data for 2004 come from the ICPSR. Exit poll data for individual states for 2008 come from CNN. *Local Exit Polls*, CNN ELECTION CENTER 2008, http://www.cnn.com/ELECTION/2008/results/polls/#ALP00p1 (last visited April 26, 2013).

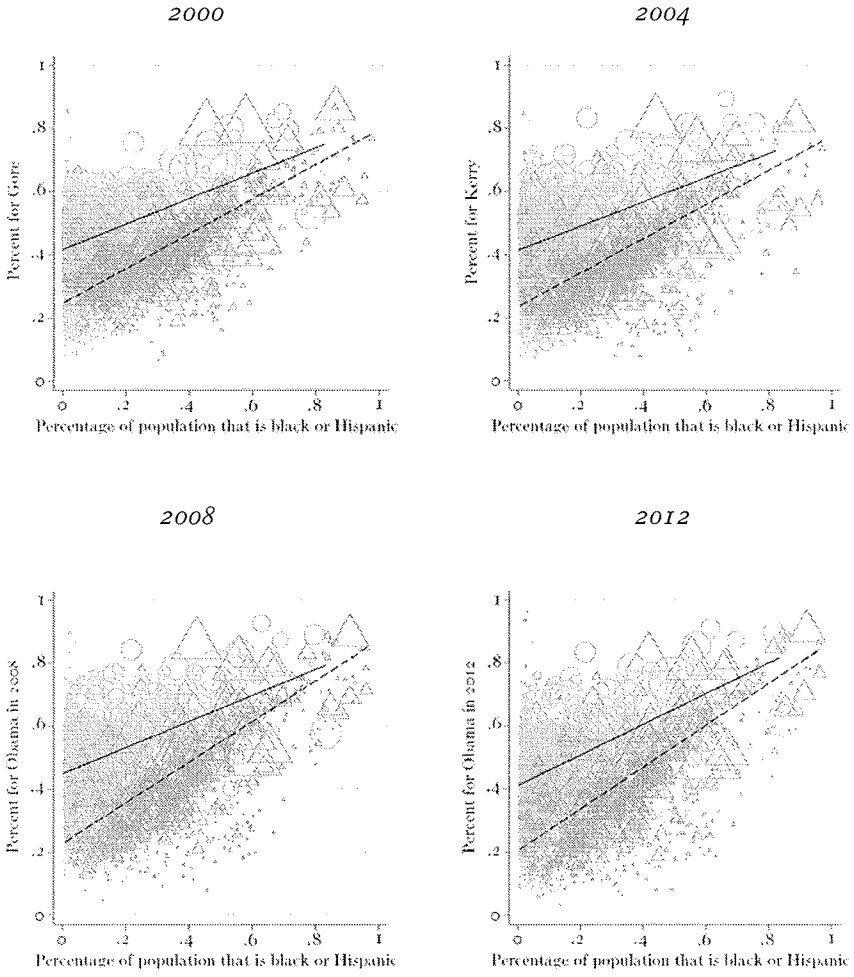
C. Racial Polarization in Presidential Elections, 2000–2012

We can further demonstrate the shifts in rates of racial polarization by analyzing the actual election returns by county and comparing them to each county's demographic makeup. This method is critical in comparing 2012 with previous years, because the national exit poll was not taken in all states in 2012. In particular, only four of the covered states (Arizona, Floria, Mississippi, and Virginia) were in the 2012 exit poll sample. In order to encompass all of the covered states, we analyze the aggregate election results, rather than the exit polls.

We can display the regressions as both a table and a graph. The three key features of the regression line are (1) the Y-intercept or constant, which indicates the likely white support for the Democratic nominee; (2) the steepness or slope of the line, which reveals how closely related the racial composition of a district is to voteshare won by the Democratic nominee (a forty-five-degree line would suggest that each one-percent increase in the black plus Hispanic share of the county's population translates into one-percent vote for the Democratic candidate); and (3) the fit or R-squared value, which indicates how good the regression line fits the data (that is, how close are the various data points to the line and therefore how easy it is to predict the Democratic voteshare when knowing only the minority population share of the county). Each measure is helpful in assessing racial polarization and comparing polarization between the covered and noncovered counties.

As is clear from Figure B and Table 3, racial polarization according to all three statistics has been increasing in the covered jurisdictions over the last twelve years. The Y-intercept (or constant) has gone lower each year: from 0.247 in 2000 to 0.198 in 2012, suggesting average white support in the covered counties has dropped from roughly 25% to just under 20%. The same cannot be said for white support in the noncovered jurisdictions, which has hovered around 41% for the period, with the exception of 2008 where Obama won about 45% of the white vote (on average) in the noncovered counties. Consistent with the fact that Obama won a higher share of the minority vote, the slope (or steepness of the regression line) and R-squared have increased considerably in the two Obama elections as compared to their predecessors, and the differences remain great between the covered and noncovered jurisdictions. This suggests that racial composition is not only a better predictor of voteshare in the covered counties than the noncovered counties, but that it is becoming an increasingly better predictor of voteshare over time. In other words, if all one knew was the racial composition of a county, one can more accurately predict the voteshare of Obama in 2012 than for any candidate in the previous three elections.

FIGURE B. DEMOCRATIC PRESIDENTIAL SUPPORT BY COUNTY MINORITY PERCENTAGE, 2000–2012[17]



[17] Racial data by county for each presidential election was calculated according to the data files from the U.S. Census Bureau. AMERICAN FACTFINDER, http://factfinder2.census.gov (last visited April 26, 2013). For the 2000 election, the 2000 decennial census was used. For the 2012 election, the 2010 decennial census was used. For the 2004 and 2008 elections, the racial composition of each county was linearly interpolated using the 2000 and 2010 data. Other methods, such as using the American Community Survey racial data, reveal similar results.

TABLE 3. REGRESSION ESTIMATES FOR COUNTY SUPPORT FOR DEMOCRATIC CANDIDATE BASED ON MINORITY POPULATION SHARE, 2000–2012[18]

	2000		2004		2008		2012	
	Covered	Non-covered	Covered	Non-covered	Covered	Non-covered	Covered	Non-covered
Black and Hispanic percentage of county population	0.549 (0.020)	0.402 (0.012)	0.541 (0.021)	0.384 (0.013)	0.645 (0.022)	0.413 (0.013)	0.677 (0.023)	0.478 (0.013)
Constant	0.247 (0.008)	0.417 (0.003)	0.235 (0.009)	0.414 (0.004)	0.228 (0.010)	0.449 (0.004)	0.198 (0.010)	0.409 (0.004)
N	844	2,269	844	2,265	844	2,265	844	2,266
R-squared	0.47	0.33	0.44	0.27	0.50	0.31	0.52	0.36

IV. ISN'T THIS ALL ABOUT PARTISANSHIP?

As striking as the above data may be, sophisticated observers might reduce these findings to the well-known story that whites in the South have been steadily fleeing the Democratic Party over time. An active debate exists in voting rights caselaw and scholarship concerning whether a high correlation between race and partisanship should allay concerns about racial polarization.[19] However, just as we detailed three years ago, so too today the difference in candidate preferences observed in the covered counties cannot be *explained away* simply by party, even if partisanship is, admittedly, a more powerful variable than race in predicting vote choice.

Our article demonstrated this in two ways. First, using the American National Election Studies, we included many other variables in regressions with the dependent variable being white voters' vote choice. Even when controlling for party, ideology, church attendance, religiosity, union membership, age, income, and education, residence in a covered state remained a statistically significant negative factor in predicting the vote choice of whites in the 2008 election, but not in the 2004 election.[20] Second, using data from the 2008 Cooperative Con-

[18] Standard errors are in parentheses. All cell entries are statistically significant. Covered counties are listed in *Section 5 Covered Jurisdictions*, U.S. DEP'T OF JUSTICE, http://www.justice.gov/crt/about/vot/sec_5/covered.php (last visited May 3, 2013), and bailed-out counties are listed in *Section 4 of the Voting Rights Act*, U.S. DEP'T OF JUSTICE, http://www.justice.gov/crt/about/vot/misc/sec_4.php#bailout_list (last visited May 3, 2013).

[19] *See* Ansolabehere et al., *supra* note 3, at 1395.

[20] *Id.* at 1428–29.

gressional Election Survey, we also examined voting in the Democratic primary election that year. After controlling for all of the factors mentioned above, we still found that whites in the covered states were less likely to vote for President Obama than for Hillary Clinton. In other words, even when limiting the analysis to Democrats — that is, taking party out of the equation — differences in the behavior of white voters in the covered and noncovered states remained.

To confirm our prior findings from the survey data and our current findings from the ecological regressions, we turn to an analysis of the relevant data from the Survey of the Performance of American Elections (SPAE). The SPAE includes approximately 200 voters from every state in the country and is chiefly used to compare the voting experience between different states. Even with roughly 9,000 respondents, individual state effects might be difficult to unearth. However, by aggregating the covered and noncovered states together we can, at least, get a sense of whether partisanship accounts for all of the racial differences between voters in the covered and noncovered states. We should also note that our findings have now been confirmed by analysis of newly available data from the 2012 Cooperative Congressional Election Survey.

As Table 4 below confirms, the race of the voter continues to constitute a statistically significant factor in determining vote choice even after controlling for party. Even in the stripped-down first regression, race plays a more important role in the covered than the noncovered states in determining vote choice, as the substantially higher R-squared demonstrates. After adding party to the regression, however, race does not "drop out." Of course, when including party, much more of the variance in vote choice can be explained for both the covered and noncovered states, but race in the 2012 election remains a statistically significant factor in vote choice.[21] The moral of the story is that differences in party identification did not account for all of the differences between racial groups in their choice of presidential candidates in 2012 or 2008.

[21] Interestingly, when controlling for party, Hispanic race is not significant in 2008 for the covered jurisdictions but becomes significant in 2012. This is no doubt due to the fact that President Obama increased his Hispanic voteshare in 2012 while losing Anglo-white voteshare.

TABLE 4. RELATIONSHIP OF RACE AND PARTY TO VOTECHOICE FOR OBAMA, 2008 AND 2012[22]

a. Linear regression, not controlling for party

	2008			2012	
	Covered	Noncovered		Covered	Noncovered
Race = black	0.64 (0.03)	0.47 (0.02)		0.68 (0.04)	0.49 (0.02)
Race = Hispanic	0.25 (0.07)	0.26 (0.03)		0.34 (0.05)	0.26 (0.03)
Constant	0.32 (0.03)	0.48 (0.01)		0.29 (0.04)	0.46 (0.02)
N	1,614	7,164		1,534	6,819
R-squared	0.27	0.06		0.34	0.07

[22] *See* PEW CTR. ON THE STATES, ELECTION ADMINISTRATION BY THE NUMBERS (2012), *available at* http://www.pewstates.org/uploadedFiles/PCS_Assets/2012/Pew_Elections_By_The_Numbers.pdf; CHARLES STEWART III, 2012 SURVEY OF THE PERFORMANCE OF AMERICAN ELECTIONS (2012), *available at* http://www.pewstates.org/uploadedFiles/PCS_Assets/2012/CharlesStewart-Day%201.pptx.

b. Linear regression, controlling for party

	2008			2012	
	Covered	Noncovered		Covered	Noncovered
Race = black	0.23	0.14		0.22	0.12
	(0.05)	(0.02)		(0.05)	(0.02)
Race = Hispanic	0.08	0.07		0.12	0.10
	(0.07)	(0.02)		(0.03)	(0.03)
Party	0.39	0.42		0.41	0.43
	(0.03)	(0.01)		(0.02)	(0.01)
Constant	0.42	0.49		0.41	0.48
	(0.03)	(0.01)		(0.03)	(0.01)
N	1,610	7,019		1,477	6,631
R-squared	0.62	0.55		0.69	0.59

IV. CONCLUSION

Reasonable people can disagree about the relevance of the 2012 election or even racially polarized voting patterns to the constitutionality of the coverage formula for section 5 of the Voting Rights Act. Indeed, we view our findings more as a response to the notion that the election and reelection of an African American President settles the constitutional question in favor of the VRA's detractors. If anything, the opposite is true. To be sure, the coverage formula does not capture every racially polarized jurisdiction, nor does every county covered by section 5 outrank every noncovered county on this score. However, the stark race-based differences in voting patterns between the covered and noncovered jurisdictions taken as a whole demonstrate the coverage formula's continuing relevance.

In particular, for those looking for a way to distinguish the covered jurisdictions from the noncovered jurisdictions, and to do so without running afoul of the "elephant whistle" problem, differential rates of racially polarized voting provide an ideal metric. There can be no doubt that the covered jurisdictions differ, as a group, from the noncovered jurisdictions in their rates of racially polarized voting. There can also be no doubt that voting in the covered jurisdictions as a whole is becoming more, not less, polarized over time.